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THE SYSTEM OF THE INTERIOR CONDUIT AND INSULATION COMPANY.

[*Report of the Committee on Science and the Arts.*]

[No. 1,485.] HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, December 31, 1890.

The Sub-Committee of the Committee on Science and the Arts, constituted by the Franklin Institute of the state of Pennsylvania, to whom was referred, for examination,

THE INTERIOR CONDUIT AND INSULATION CO.'S SYSTEM,

respectfully report that they have carefully examined the same and report on it as follows:

The system is the subject of letters-patent of the United States:

No. 176,784, May 2, 1876. No. 359,726, Mar. 22, 1887.
No. 343,087, June 1, 1886. No. 401,498, April 16, 1889.
No. 354,320, Dec. 14, 1886. No. 435,897, Sept. 2, 1890.

No. 435,898, Sept. 2, 1890. No. 441,839, Dec. 2, 1890.
No. 441,835, Dec. 2, 1890. No. 441,840, Dec. 2, 1890.
No. 441,836, Dec. 2, 1890. No. 441,845, Dec. 2, 1890.
No. 441,837, Dec. 2, 1890. No. 441,846, Dec. 2, 1890.
No. 441,838, Dec. 2, 1890. No. 441,870, Dec. 2, 1890.
No. 441,090, Dec. 2, 1890.

The purpose of the invention is to provide a means for running electric light wires in and through buildings and

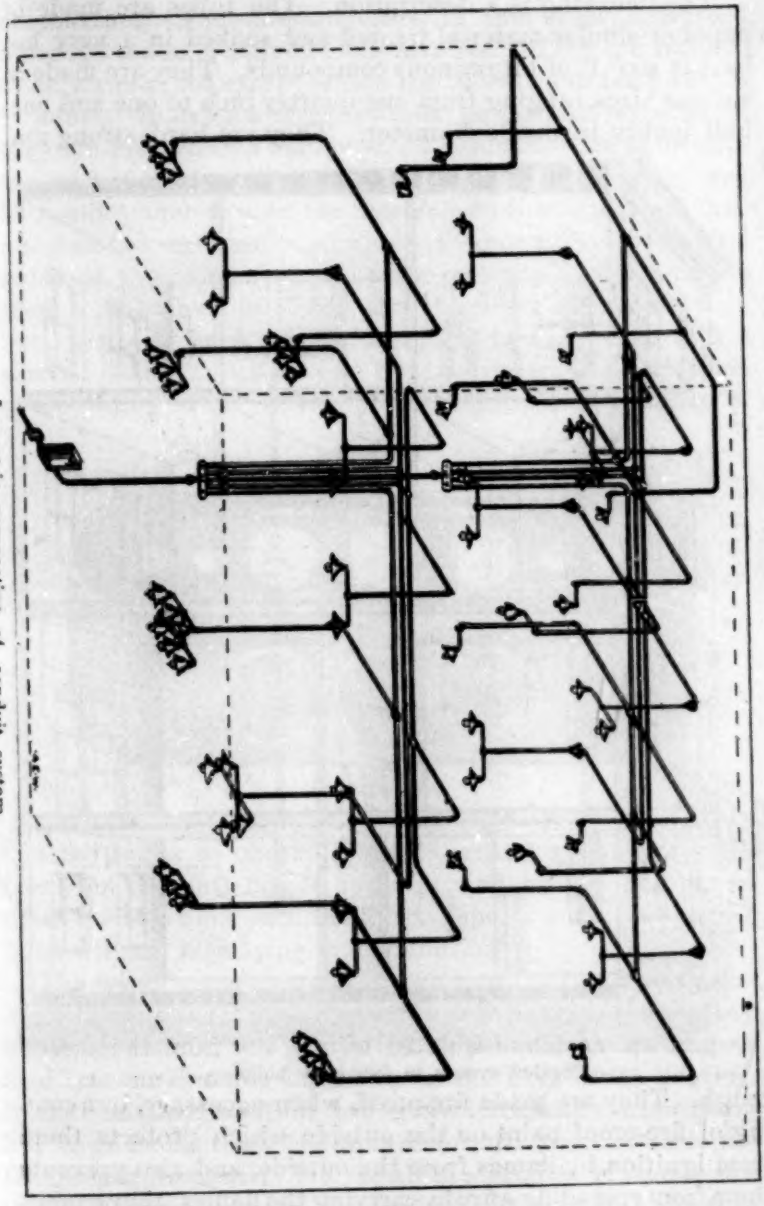
New coupling.

underground. As it differs essentially from the usual system of running such wires, it may be said to be to a certain extent a new departure, rather than an improvement on the usual.

In general, it consists of a system of continuous tubes of insulating material, which are placed throughout a building, into which tubes the wires are afterwards drawn. The

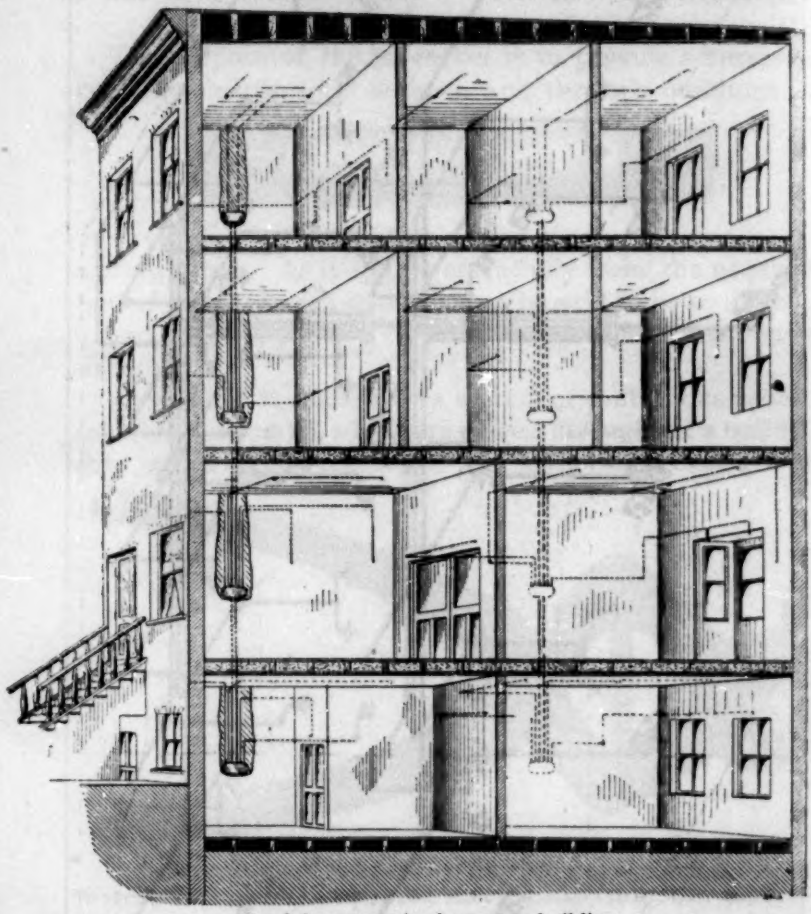
Coupling tool.

wires are thereby insulated electrically from the materials of the building, such as plaster, wood or stone-work, gas and water pipes, etc., while the wood-work of the building is also protected from being ignited by the accidental heating or short-circuiting of the wires themselves, thus protecting the building against liability to fires arising from such accidents.



Isometric perspective of conduit system.

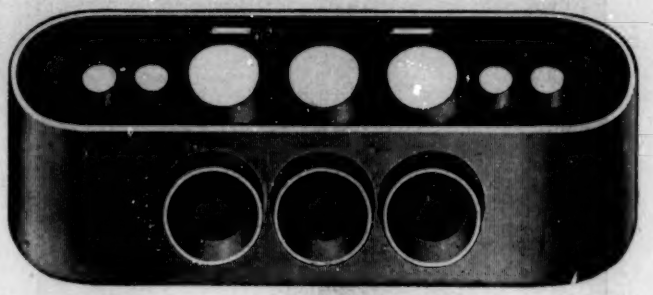
The following is a description. The tubes are made of paper or similar material treated and soaked in a very hot bath of 460° F. of bituminous compounds. They are made in various sizes, ranging from one-quarter inch to one and one-half inches in inside diameter. They are hard, strong and



Conduit system in four-story building.

tough. They are made fire-proof, when necessary, by a coating of fire-proof paint on the outside which protects them from ignition by flames from the outside, and also prevents them from spreading a fire by carrying the flames; they cannot be ignited from the inside for want of sufficient air to support

combustion. They are therefore, in this sense, fire-proof, although made of inflammable material. They are claimed to be water-proof except where the tubes have been cut, as at joints. In cases where they must be absolute water-proof they are incased in a lead pipe. These tubes are secured to the wall of the building by a simple brass clip. They may be run in under or over the plaster. In the latter case they are finished externally so as to accord with the general finish of the wood-work or other ornamental work in the room. At corners of bends, the tubes are connected with butt joints to knee pieces, the joint being enclosed in a sleeve. These tubes are laid from the centre of distribution of a building to the outside of the fixtures. The centre of



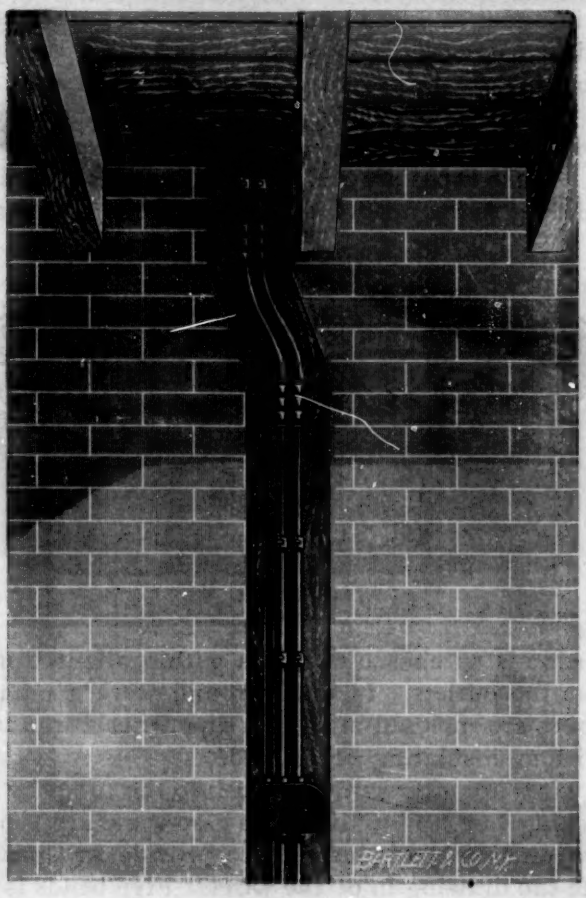
Junction box.

distribution is so chosen that it is within about thirty-five feet from the outlets. In underground work a number of these tubes are enclosed in an iron pipe, or cast-iron boxing, filled with an insulating compound.

The wires are afterwards drawn into the tubes by means of a "fishing" or "pilot" line. In case there are too many bends, a junction box is inserted to provide an opening to facilitate the drawing in of the wires. Five bends is given by the company as a limit through which a wire can be drawn. For large mains there is one tube for the positive and one for the negative main, but for all the smaller branches the two wires are run in the same tubes. These wires, whether double or single, have merely a light textile insulation, in

place of the usual expensive fire and water-proof coverings. That no danger arises from using such simple insulation was shown by the committee by experiments described below.

Among the merits of the invention are the following: It affords a more thorough insulation of the wires from the



Wall with risers.

building and protects the wires themselves from injury from external sources, such as moisture, abrasion, or other mechanical injury. It protects the building from fire arising from overheated wires, and thereby overcomes a great

Specimen board.

source of danger. It protects a building against fires arising from imperfect workmanship in running the wires, as loose joints, for instance. As the tubes are independent to a great extent, of the particular system of distribution to be used, it permits them to be laid when the building is constructed, the wires for any particular system being drawn into them when required. This overcomes an important objection to the usual system of the laying of the wires themselves into new buildings before it is known what system of distribution may be subsequently introduced. It also permits that the wires may be withdrawn and renewed in case they have been burnt out, or they may be replaced by large ones when it is desired to increase the number of lights at any subsequent time. The underground system was not exhibited to this committee, and is therefore not included in the report.

The following experiments were shown to the committee: A leak was forced between two wires, run, as is usual, on a wooden moulding, two and one-half inches apart, the leak was made, moistening the moulding with a solution of soda. The leakage was too small to actuate the safety fuses, but was large enough to char and set fire to the wood. The wires were placed close together, as in the system of tubes and the leak was produced as before. The leakage was so great that the safety fuses acted immediately, before any charring or ignition of the wood had time to take place. In another experiment a wire was secured to a piece of wood, and a continuation of the same wire was passed through the tubes; the whole wire was then overloaded so as to cause it to heat. The wood was set on fire, but the tube was not hot enough to do any harm, although the insulation around the wire itself had been completely consumed by the heat. Other experiments were made showing that a short circuit in a tube would not start a fire, while a similar short circuit in the open air or moulding is very apt to be a source of fire.

The question of the scope or extent of the patents was not considered by the committee.

Your committee endorse the merits of the above invention and add that in their opinion it is an improvement of importance and value. In view of these facts they recom-

mend the award of the John Scott Legacy Medal and Premium.

CARL HERING, *Chairman,* RICHARD W. DAVIDS,
T. CARPENTER SMITH, E. A. SCOTT,
WM. MCDEVITT,

Adopted, February 4, 1891.

S. L. WIEGAND,
Chairman of the Committee on Science and the Arts.

THE SYSTEM OF HOUSE AND UNDERGROUND WIRING OF THE INTERIOR CONDUIT AND INSULATION COMPANY.

BY E. H. JOHNSON, New York.

[*A Paper read before the Franklin Institute, December 17, 1890.*]

JOSEPH M. WILSON, president, in the chair.

MR. JOHNSON—MR. PRESIDENT AND MEMBERS OF THE INSTITUTE:

Having been identified with the new industry of incandescent electric lighting, and more particularly with the practical work of distributing the electric current therefor, since its inception, I have been made aware of the requirements of the problems of safety, convenience and economy in a pronounced way, and in the system which I am honored in being requested to bring to your attention to-night, I claim to have fully incorporated these essential factors.

Briefly described, the system is nothing more or less than a well worked-out scheme of supplanting or supplementing the former practice of attaching the insulation to the wire itself, by a system of insulated raceways, into which to draw the wires, and which supplies in itself a degree of high and durable insulation not attainable by the old methods, while supplying in addition the highly important (I might almost say imperative) adjuncts of accessibility.

In the matter of safety, I have only to call your attention to the fact that it is practically impossible to main-

tain a flame within the circumscribed limits of a tube scarcely greater in diameter than the material ignited within it; such an enclosure becomes a snuffer, and this quality of suffocating combustion is independent of the inflammability of the material of which the snuffer may be composed. No elaborate description of the system is required. The sample of wiring which stands before you tells its own story on the instant, and the experiments which my assistant and associate inventor, Mr. Greenfield, will now make, will demonstrate its utility better than any verbal explanation Before proceeding to such demonstration, however, I will make mention of one or two points which an experience of a year in promoting this system has developed.

(1) You will readily perceive that if this means of insulating wires for interior or underground purposes should in practive prove all-sufficient, the very large interests now invested in the wire-insulating industries would imagine itself seriously jeopardized, and the new method would encounter the open and severe attack of such interest. This attack has been made, and while I have not claimed for the system that it is a complete substitute for all other insulations, I have, by virtue of the very efforts that have been made to depreciate it, been led to develop it on lines which will speedily enable it to fulfil such conditions absolutely.

In illustration, certain rubber insulation interests in the city of Boston, undertook to prove to interested authorities that the tube would burn, not by virtue of heat emanating from the wires within it, but by fires originating from other and external causes; and this demonstration was made quietly and without my knowledge. Upon learning the facts, I at once called attention to the fact that no claim of non-combustibility had ever been made for it. That such was, in fact, not an essential requisite, nevertheless, I then undertook to provide a special tube which might be employed in certain cases where this feature was important. A sample of this tube will be shown you to-night.

(2) It has been alleged also that this tube disintegrates

in time in the presence of moisture. This criticism was very properly made against the first tube we placed on the market, but when the new tube completely met it, the criticism was not abandoned but was kept alive by the exhibitions of the early samples. When, however, in the course of time, I made this contention untenable, it was narrowed down to the statement that the tube would disintegrate in cement. Upon investigation I found this to be true and endeavored at once to correct it if possible. I am glad to say that this most difficult of all problems has now been satisfactorily solved.

(3) It has been asserted that its cost, in consequence of its not being a complete substitute for other insulation, is simply an additional charge on an installation and one that brings no sufficient compensations. To this I reply that the cost of an installation by this method need be no greater than that of any other method approved by the better class of underwriter inspectors. Not so great, in fact, as some methods. The difficulty is that too much work is done, where reliance is placed wholly on the insulation of the wire itself. In fact, notwithstanding the universal knowledge that no insulated wire exists which does not need to be physically protected, many installations are thus made to-day and pass the inspectors, where owners and architects, however, take an intelligent interest in the subject and insist upon superior work. This system can be and is installed at prices that compete with others.

Furthermore, the details of its application are being cheapened and improved every day, and it is probably not even now too much to claim for the system that its opponents have caused it to be developed to a degree that will justify the contention that their wires may be safely employed in it and thus the tables be turned; that is to say, it may be installed more cheaply than any other, because the costly insulation of wires by its use are dispensed with.

I venture, in conclusion, to submit a statement and description of a new underground conduit system, which is more or less the outgrowth of the interior conduit system just described, and is the result of a series of costly and

elaborate experiments to determine the practical requirements of underground electric conductors. The system is not unlike the interior system, in that it consists of a series of highly insulating, water-proof tubes, which form individual channels or raceways for the reception of whatsoever electrical conductors may be employed, thus providing for the drawing in, or withdrawal of, conductors at any time, rendering them as accessible for renewal and repairs as if placed on the surface itself.

In its physical characteristics it differs from the ordinary iron, wood, cement and other conduits, in that while they only contemplate ducts of uniform and large diameter, each accommodating a considerable number of wires, these conduits provide an individual duct of suitable dimensions for each and every wire or cable employed. In the important element of insulation, however, it differs radically. All conduits hitherto employed practically ignore the problem of insulation, thus compelling the use of costly and cumbersome insulations upon the wires themselves, whereas this system provides in the conduit itself without special investment therefor an insulation ample for all purposes. The extra charge, direct and indirect, incurred for the necessarily expensive insulations and metallic protections of such insulated cables and for handling their greater bulk and weight, will, in any plant of material size, amount to a sum more than sufficient to pay the entire cost of this conduit system.

This conduit may consist of any desired number of individual tubes or ducts, continuous from one man-hole to another, no joints or couplings being employed to unite the several sections of the individual ducts, and thus a fruitful cause of leakage and consequent deterioration is eliminated. The method of converting the tube sections into continuous unbroken and unjointed ducts constitutes one of the important elements of our system.

The general method is as follows:

A series of tubes of any given number, length and diameter is racked in a suitable frame so as to be held at a uniform distance apart. The segregated bundle thus

formed is then pushed into a cast-iron pipe of a diameter slightly greater than that of the bundle of tubes, but of exactly the same length. A jig is then attached to the end to secure a uniform terminal location of the ducts, and the whole is then immersed in a bath of special insulating compound maintained at a high temperature. When withdrawn from this bath, only that portion of the compound which has entered the interior of the tubes is allowed to escape, all that portion which has filled the waste interstices is imprisoned, and subsequently becomes solidified, thus presenting a substantial body composed only of a high insulating material, throughout which are the clear, straight and smooth duct interiors; these completed sectional bodies are for convenience of handling manufactured in ten-foot lengths, and in this form are ready for laying. Realizing, however, that all attempts at hermetically uniting the multiple ducts of such sectional systems have proven utter and disastrous failures, we have evolved a method of laying these conduits, which converts these sectional units into continuous ducts having practically no junctions, certainly none of inclusive cross section, or contiguous coincidence. The manner in which this is effected is as follows:

In addition to the consolidated section above described, we employ individual tubes the length of which is the same as the consolidated ducts, the external diameter of which, however, is slightly less than the internal diameter of the consolidated ducts and the internal diameter of which is just what is called for by the requirements of the service for which they are intended. These individual tubes are coated at the moment of laying with an insulating compound or cement, and are then telescoped into the consolidated ducts to the depth of just one-half their length in each section. The abutting ends of the exterior or consolidated tubes of contiguous sections are, of course, mid-way of the general containing pipes, each and every duct thus telescopically formed of the two complete tubes and cemented with a superior insulating compound, becomes, therefore, to all intents and purposes as perfect in its continuity as if manufactured at the factory in one unbroken

length. The general containing pipes are united by a special sleeve or coupling box, which is thoroughly filled with insulating compound and further sealed by the insertion of a sheet-lead gasket between the sleeve or box and the abutting ends of the pipe.

From man-hole to man-hole, we thus provide a system which in its outer material form and appearance is identical with that method, which fifty years of practical experience has evolved as best practice in the distribution of water, gas, steam, etc. Cast iron having become the accepted economical and durable channel for the distribution of energy in all its other forms, why not for the distribution of electricity? The answer to this question has hitherto been found in the absence of suitable provision for the continuity of insulation in each and every one of the separate ducts demanded by the peculiar requirements of electrical distribution, viz: That the aggregate volume traversing a given channel must consist of separately maintained minor units or volumes, each of which possesses some special characteristic of pressure or other function differentiating it from its neighbor. This problem of absolutely reliable insulating continuity is now solved for the first time by our telescopically continuous insulated ducts.

Man-holes.—Having applied to electrical channel distribution the best practice evolved by other and older industries, and having only supplemented these with new devices where the special requirements of electric distribution demanded, we have proceeded to analyze the terminal requirements of electrical conductors and, as far as possible, have endeavored to apply thereto that method of practice which experience has demonstrated to be most effective. The "man-hole" is practically a creation of the electrical industry; we are therefore limited in any consideration of its problems to electrical practices; fortunately, however, the man-hole is an indispensable adjunct of all electrical systems, hence, by virtue of the extraordinary development of the electrical industry, we become possessed of a vast deal of practical experience from which to obtain useful data. An examination of these data develops the fact that the

demand is for a man-hole which may be left open with impunity. This means simply that the conductors passing through it and the ducts entering it must be considered, in so far as their treatment is concerned, as though permanently terminating in a cesspool. We have attacked the problem from this point of view.

The man-hole as we have finally designed it is simply a construction of brick and carefully cemented to protect it as far as possible from the exudations of the soil, for the sake of cleanliness. The section of the general containing pipe entering the man-hole is expanded in a funnel form to about double its normal diameter, for the purpose of separating, and, therefore, providing a greater space around the duct ends for more perfect manipulation. The end of this funnel is provided with an iron head, on the face of which are short pipe projections corresponding to the number of ducts, and into each of which a duct end terminates. These iron nipples are threaded on the outside for the reception of a metallic cap or coupling. When the conductor has been drawn into the duct a section of lead pipe with an insulated lining is pushed over the end of the conductors, and a short distance into the duct itself; a coupling is then screwed onto the cap and over the lead-encased conductor and so firmly screwed home as practically to consolidate the lead sheathing and iron nipples, and thus hermetically seal the end of the duct. The corresponding end of the conductor on the opposite side of the man-hole having been treated in the same way, it now only remains to join the broken sections of the conductors and their lead sheathing, hang the completed conductor upon its proper rack at the side of the man-hole, and proceed to treat each of the other conductors in a like manner. When all are so treated, and any unoccupied ducts securely sealed, the man-hole itself may be left open with impunity.

It will be observed that since each and every conductor is treated at the man-hole as flexible cable, and hung upon a rack at its side, the centre of the man-hole remains unobstructed, and perfect freedom is afforded the workmen who may need to get at any particular conductor for purpose of testing, repairs or lateral tapping.

Lamp-posts, feeder-posts, or other branch taps are provided, and are so constructed as to be nothing more or less than minor extensions of the system in its every feature. They are iron pipes filled with our insulating compound and jointed to the conduit proper by suitably constructed elbow extensions, thus providing for the drawing in of the conductors from the nearest man-hole to the top or extreme terminal of the post or other branch.

In conclusion, I may say that we feel free to express our conviction that in point of first cost, durability, flexibility, insulation, cleanliness, economy of maintenance, economy of space and general adaptability, no other combination of underground conduit and conductors can even approximately equal this system.

I thank you, Mr. President and gentlemen, for your courteous attention.

THE CONTINUOUS GIRDER—VARIABLE MOMENT OF INERTIA—FIXED POINTS—GRAPHIC METHOD.

BY C. H. LINDENBERGER.

Those who are familiar with graphic methods would scarcely like to lose the sense of assured satisfaction that comes from proving the agreement of a graphic calculation with an analytical one, even though the latter may not be so very difficult. This is the case in the majority of instances, but there is this advantage that an increased difficulty in the analytical calculation seldom means a corresponding difficulty in the graphical method. Hence a graphic theory is always valuable as a means of solution.

The theory of solution by "fixed points," so far as it relates to "constant moment of inertia," has been dealt with in this journal for December, 1888, and November, 1889. It remains to extend it to "variable moment of inertia," and, to make the theory general, to extend it to the case in which the coëfficients of the unknown quantities are different in each equation.

Thus, for instance, let there be a series of equations like this to be solved:

$$\begin{aligned} Q_2 M_2 + R_2 M_3 &= Y_2 \\ U_2 M_2 + Q_3 M_3 + R_3 M_4 &= Y_3 \\ \text{etc., etc., etc., etc.} & \qquad (1) \\ U_{s-2} M_{s-2} + Q_{s-1} M_{s-1} + R_{s-1} M_s &= Y_{s-1} \\ U_{s-1} M_{s-1} + Q_s M_s &= Y_s \end{aligned}$$

There are $(s-1)$ of these equations, and also $(s-1)$ unknown quantities denoted by M with their proper subscripts. All the other quantities are known.

Hence the problem is soluble by ordinary algebra, and is, in fact, in all of its phases nothing but algebra, though in practice the continuous girder furnishes the problem.

Multiply the first equation by D_2, the second by D_3, etc., add and arrange as coëfficients of the unknown quantities, and we have

$$\left.\begin{aligned} & M_2 (D_2 Q_2 + D_3 U_2) - D_2 Y_2 \\ + & M_3 (D_2 R_2 + D_3 Q_3 + D_4 U_3) - D_3 Y_3 \\ & \text{etc., etc., etc., etc.} \\ + & M_{s-1} (D_{s-2} R_{s-2} + D_{s-1} Q_{s-1} + D_s U_{s-1}) - D_{s-1} Y_{s-1} \\ + & M_s (D_{s-1} R_{s-1} + D_s Q_s) - D_s Y_s \end{aligned}\right\} = o \qquad (2)$$

Assume that the coëfficient of $M_q = Z_q$, and that of all the others $= o$, and we have

$$M_q = \frac{D_2 Y_2 + D_3 Y_3 + \text{etc.}, + D_{s-1} Y_{s-1} + D_s Y_s}{Z_q} \qquad (3)$$

$$D_{q-1} R_{q-1} + D_q Q_q + D_{q+1} U_q = Z_q \qquad (4)$$

By reason of this assumption we have here $(s-1)$ equations and $(s-1)$ unknown quantities of the type D and Z_q. If we assume that $D_2 = 1$, all the rest of these quantities become determinate.

It is better, however, to ascertain their values differently.

Assume the following equations:

$$\begin{aligned} c_1 R_1 + c_2 Q_2 + c_3 U_2 &= o \\ c_2 R_2 + c_3 Q_3 + c_4 U_3 &= o \\ \text{etc., etc., etc., etc.} & \qquad (5) \end{aligned}$$

$$c_{s-2} R_{s-2} + c_{s-1} Q_{s-1} + c_s U_{s-1} = o$$
$$c_{s-1} R_{s-1} + c_s Q_s + c_{s+1} U_s = o$$

There are $(s-1)$ of these equations, and by assuming that $c_1 = o$, $c_2 = 1$ all the unknown quantities of the type c become determinate.

Write d for c in the above equations and assume that $d_{s+1} = o$, $d_s = 1$ then all the unknown quantities of the type d become determinate.

By examination of the series of equations (5) it is evident that the equations for the determination of D_n, where n is equal to or less than q have the same form as those by which c_n is found, and, moreover, $D_2 = c_2 = 1$. Hence

$$D_n = c_n \qquad (6)$$
$$\text{for } n = \text{or} < q$$

These equations, as modified by the substitution of d for c, also show a series of the same form for the determination of

$$\frac{D_n}{D_s} \text{ and } \frac{d_n}{d_s}$$

if n is either equal or greater than q. Hence

$$\frac{D_n}{D_s} = \frac{d_n}{d_s} = d_n \text{ since } d_s = 1$$
$$\text{for } n = \text{or} > q$$
$$\text{and making } n = q, \text{ equation (6)}$$
$$D_s = \frac{c_q}{d_q}$$

whence

$$D_n = \frac{c_q}{d_q} d_n \qquad (7)$$
$$\text{for } n = \text{or} > q$$

These values in equation (4) give us

$$c_{q-1} R_{q-1} + c_q Q_q + \frac{c_q}{d_q} d_{q+1} U_q = Z_q \qquad (8)$$

and we also have

$$c_{q-1} R_{q-1} + c_q Q_q + c_{q+1} U_q = o \qquad (9)$$
$$d_{q-1} R_{q-1} + d_q Q_q + d_{q+1} U_q = o \qquad (10)$$

Equation (8) becomes from equation (9)

$$(c_q\, d_{q+1} - c_{q+1}\, d_q)\, U_q = d_q\, Z_q \qquad (11)$$

Multiply equation (9) by d_q and equation (10) by c_q and subtract the latter, and we have

$$(c_{q-1}\, d_q - c_q\, d_{q-1})\, R_{q-1} - (c_q\, d_{q+1} - c_{q+1}\, d_q)\, U_q = o$$

which may be written

$$\frac{R_{q-1}}{U_{q-1}}\,(c_{q-1}\, d_q - c_q\, d_{q-1})\, U_{q-1} = (c_q\, d_{q+1} - c_{q+1}\, d_q)\, U_q$$

or from equation (11)

$$\frac{R_{q-1}}{U_{q-1}}\, d_{q-1}\, Z_{q-1} = d_q\, Z_q \qquad (12)$$

It thus appears that $d_q\, Z_q$ is not a constant quantity, for by hypothesis R_{q-1} is different from U_{q-1}. If, however, this were not the case; that is, if the rule were universal that $U_n = R_n$, then we would have

$$d_q\, Z_q = d_{q-1}\, Z_{q-1} = d_s\, Z_s = Z_s \qquad (13)$$

This is the case with the usual form of equations for "constant moment of inertia."

The original equations may be transformed so that new coëfficients are evolved in which this rule would hold good; that is, the new value of U_n will be equal to the new value of R_n. This is done in the following manner:

Multiply the first of the original equations (1) by $z_1\, z_2$, the second by $z_2\, z_3$, etc., and assume the following equations:

$$z_2\, z_3\, U_2 = z_1\, z_2\, R_2,$$

etc., etc., etc.

$$z_{s-1}\, z_s\, U_{s-1} = z_{s-2}\, z_{s-1}\, R_{s-1}$$

The number of these equations is $(s-2)$. Hence, if we assume the value of z_1 and z_2, we can find the value of the other unknown quantities of the type z, and thence the value of the new coëfficients.

This, however, is not necessary in the present instance, for the original equation for the continuous girder of vari-

able moment of inertia has been shown by the previous article to take the following form:

$$M_{r-1} L_{r-1} + M_r (T_r + G_{r-1}) + M_{r+1} L_r = Y_r \qquad (14)$$

where

$$Y_r = A_r + B_{r-1} - \left(\frac{h_r - h_{r+1}}{l_r} + \frac{h_r - h_{r-1}}{l_{r-1}} \right) \qquad (15)$$

M is the moment at the support denoted by its subscript and h_r is the vertical ordinate of that support. The quantities L_r G_r T_r A_r and B_r have been defined in this *Journal*, January, 1891, but the definition is too tedious to be given here. l_r is the length of the r^{th} span.

Equations (9), (10), (11) and (13) become, respectively substituting r for q,

$$c_{r-1} L_{r-1} + c_r (T_r + G_{r-1}) + c_{r+1} L_r = o \qquad (9a)$$

$$d_{r-1} L_{r-1} + d_r (T_r + G_{r-1}) + d_{r+1} L_r = o \qquad (10a)$$

$$(c_r d_{r+1} - c_{r+1} d_r) L_r = d_r Z_r = Z_s \qquad (11a)$$

$$d_r Z_r = d_{r-1} Z_{r-1} = d_s Z_s = Z_s \qquad (13a)$$

and from equations (6) and (7) and (13a) we deduce

$$M_r = \frac{d_r (c_2 Y_2 + c_3 Y_3 \text{ etc.} + c_r Y_r) + c_r (d_{r+1} Y_{r+1} + \text{etc.} + d_s Y_s)}{Z_s} \qquad (16)$$

which is the same form as that given in this *Journal* for December, 1888.

$$Z_s = c_{s-1} L_{s-1} + c_s (T_s + G_{s-1}) \qquad (17)$$

It should be remarked that the girder has s spans, and therefore $(s+1)$ supports. We cannot therefore in equation (14) have either $r = 1$ or $r = s + 1$. An obvious reason is, among others, that we should only have as many equations as there are unknown moments to be determined since the moments at the end supports $= o$. Of course, this applies to equation (14) only, and not to the rest.

For the purpose of a graphical solution let us assume that in the r^{th} span there are two "fixed points," whose

tance from the r^{th} support is given by the following equations:

$$i_r = \frac{c_r l_r}{c_r - c_{r+1}} \tag{18}$$

$$I_r = \frac{d_r l_r}{d_r - d_{r+1}} \tag{19}$$

These points have been called "fixed inflection points," or "points of zero moment." Now, while either of these names are not misnomers they have all the effect of it.

They were originally so called because they are in certain cases, inflection points or points of zero moment for loads or disturbances of level in other spans, but the names tend to create the impression that they are *always* such under all circumstances, even among those who ought to know better. I prefer, therefore, simply to call them "fixed points."

By the aid of equations (18), (19) and (11*a*), we obtain

$$Z_s = c_r\, d_r \left(\frac{I_r - i_r}{I_r\, i_r}\right) L_r\, l_r \tag{20}$$

I will now show how these fixed points are obtained for either constant or variable moment of inertia. In *Fig.* 1,

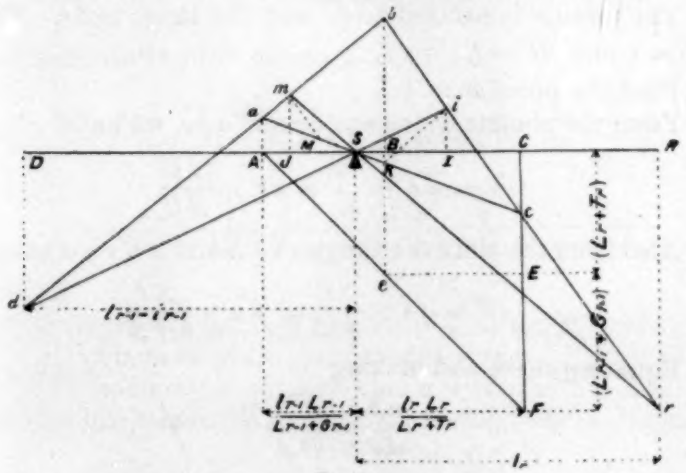


FIG. 1.

Let S be the r^{th} support.

Let $$\overline{DS} = l_{r-1} - i_{r-1} = -\frac{c_r l_r}{c_{r-1} - c_r}$$

Let $$\overline{SR} = I_r = \frac{d_r l_r}{d_r - d_{r+1}}$$

$$\overline{AS} = \frac{l_{r-1} L_{r-1}}{L_{r-1} + G_{r-1}}$$

$$\overline{SC} = \frac{l_r L_r}{L_r + T_r}$$

$\overline{CE}$ = or proportional to $(L_r + T_r)$
$\overline{EF}$ = " " $(L_{r-1} + G_{r-1})$

The lengths $\overline{AS}$ and $\overline{SC}$ have been graphically obtained in the previous article, and are found from the intersection of the cross lines formed by the prolongation of the end sides of the equilibrium polygons required for the graphical calculation of L_r T_r and G_r.

Draw vertical lines through all these points. Take $\overline{Dd}$ any convenient length and bd any convenient slope, and this will determine the length of $\overline{Rr}$. Complete the figure. The lines $\overline{ik}$ and $\overline{mM}$ are not drawn in practice, but are necessary for the demonstration.

The former is parallel to $\overline{bd}$ and the latter to $\overline{br}$. Then $\overline{SI} = i_r$ and $\overline{SJ} = l_{r-1} - I_{r-1}$.

First the proof as to $\overline{SI}$.

From the similar triangles $\overline{ikc}$ and $\overline{abc}$, we have

$$\overline{ik} = \overline{ab} \times \frac{\overline{ic}}{\overline{bc}} = ab \times \frac{\overline{IC}}{\overline{BC}}$$

Also from the similar triangles $\overline{ikS}$ and $\overline{dSa}$ we have

$$\overline{ik} = \overline{da} \times \frac{\overline{Si}}{\overline{dS}} = \overline{da} \times \frac{\overline{SI}}{\overline{DS}}$$

Equating these and making

$$\frac{\overline{ab}}{\overline{da}} = \frac{\overline{AB}}{\overline{DA}}$$

$$\frac{\overline{SI}}{\overline{DS}} = \frac{\overline{AB}}{\overline{DA}} \cdot \frac{\overline{IC}}{\overline{BC}} = \frac{\overline{AB}}{\overline{DA}} \left(\frac{\overline{SC} - \overline{SI}}{\overline{BC}} \right)$$

whence

$$\overline{SI}(\overline{DA}\cdot\overline{BC}+\overline{AB}\cdot\overline{DS})=\overline{AB}\cdot\overline{SC}\cdot\overline{DS}$$

Now

$$\overline{DA}=\overline{DS}-\overline{AS}$$

and

$$\overline{BC}(\overline{DS}-\overline{AS})+\overline{AB}\cdot\overline{DS}=\overline{DS}\cdot\overline{AC}-\overline{AS}\cdot\overline{BC}$$

whence

$$\overline{SI}=\frac{\overline{AB}\cdot\overline{SC}\,\overline{DS}}{\overline{DS}\cdot\overline{AC}-\overline{AS}\cdot\overline{BC}}=\frac{\overline{SC}}{\frac{\overline{AC}}{\overline{AB}}-\frac{\overline{BC}}{\overline{AB}}\cdot\frac{\overline{AS}}{\overline{DS}}}$$

$$=(\text{by similar triangles})=\frac{\overline{SC}}{\frac{\overline{CF}}{\overline{CE}}-\frac{\overline{EF}}{\overline{CE}}\cdot\frac{\overline{AS}}{\overline{DS}}}$$

$$\left(\text{because }\frac{\overline{BC}}{\overline{AB}}=\frac{\overline{EF}}{\overline{CE}}\right)$$

$$=\frac{\overline{CE}\cdot\overline{SC}}{\overline{CF}-\frac{\overline{EF}\cdot\overline{AS}}{\overline{DS}}}$$

and substituting the values

$$\overline{SI}=\frac{l_r L_r}{L_r+T_r+L_{r-1}+G_{r-1}+L_{r-1}\,l_{r-1}\,\frac{(c_{r-1}-c_r)}{c_r\,l_{r-1}}}$$

$$=\frac{c_r L_r l_r}{c_r(L_r+T_r+L_{r-1}+G_{r-1})+c_{r-1}L_{r-1}-c_r L_{r-1}}$$

but

$$c_{r-1}L_{r-1}+c_r(T_r+G_{r-1})=-c_{r+1}L_r$$

whence

$$\overline{SI}=\frac{c_r l_r}{c_r-c_{r+1}}=i_r \qquad (21)$$

which was to be proved.

The next is the proof as to $\overline{SJ}$.

From the similar triangles $\overline{m\,M\,a}$ and $\overline{a\,c\,b}$ we have

$$\overline{m\,M} = \overline{b\,c} \cdot \frac{\overline{a\,m}}{\overline{a\,b}} = \overline{b\,c} \cdot \frac{\overline{A\,J}}{\overline{A\,B}}$$

Also from the similar triangles $\overline{m\,M\,S}$ and $\overline{r\,S\,c}$, we have

$$\overline{m\,M} = \overline{c\,r} \cdot \frac{\overline{S\,m}}{\overline{S\,r}} = \overline{c\,r} \cdot \frac{\overline{S\,J}}{\overline{S\,R}}$$

Equating these and putting

$$\frac{\overline{b\,c}}{\overline{c\,r}} = \frac{\overline{B\,C}}{\overline{C\,R}}$$

we get

$$\frac{\overline{S\,J}}{\overline{S\,R}} = \frac{\overline{B\,C}}{\overline{C\,R}} \cdot \frac{\overline{A\,J}}{\overline{A\,B}} = \frac{\overline{B\,C}}{\overline{C\,R}} \left(\frac{\overline{A\,S} - \overline{S\,J}}{\overline{A\,B}} \right)$$

$$\overline{S\,J}\,(\overline{C\,R} \cdot \overline{A\,B} + \overline{B\,C}\,\overline{S\,R}) = \overline{B\,C} \cdot \overline{A\,S} \cdot \overline{S\,R}$$

Now

$$\overline{C\,R} = \overline{S\,R} - \overline{S\,C}$$

and

$$\overline{A\,B}\,(\overline{S\,R} - \overline{S\,C}) + \overline{S\,R} \cdot \overline{B\,C} = \overline{S\,R} \cdot \overline{A\,C} - \overline{A\,B}\,\overline{S\,C}$$

whence

$$\overline{S\,J} = \frac{\overline{B\,C} \cdot \overline{A\,S} \cdot \overline{S\,R}}{\overline{S\,R} \cdot \overline{A\,C} - \overline{A\,B} \cdot \overline{S\,C}} = \frac{\overline{A\,S}}{\frac{\overline{A\,C}}{\overline{B\,C}} - \frac{\overline{A\,B}\,\overline{S\,C}}{\overline{B\,C}\,\overline{S\,R}}}$$

$$\text{(by similar triangles)} = \frac{\overline{A\,S}}{\frac{\overline{C\,F}}{\overline{E\,F}} - \frac{\overline{C\,E}}{\overline{E\,F}} \cdot \frac{\overline{S\,C}}{\overline{S\,R}}}$$

$$= \frac{\overline{E\,F} \cdot \overline{A\,S}}{\overline{C\,F} - \frac{\overline{C\,E} \cdot \overline{S\,C}}{\overline{S\,R}}}$$

and by substituting the values

$$\overline{S\,J} = \frac{l_{r-1}\,L_{r-1}}{L_r + T_r + L_{r-1} + G_{r-1} - l_r\,L_r\,\frac{(d_r - d_{r+1})}{d_r\,l_r}}$$

$$= \frac{d_r\,l_{r-1}\,L_{r-1}}{d_r\,(L_r + T_r + L_{r-1} + G_{r-1}) - d_r\,L_r + d_{r+1}\,L_r}$$

Now

$$d_r (T_r + G_{r-1}) + d_{r+1} L_r = - d_{r-1} L_{r-1}$$

whence

$$\overline{SJ} = \frac{d_r l_{r-1}}{d_r - d_{r-1}} = l_{r-1} - l_{r-1} \qquad (22)$$

which was the solution sought.

The reader may possibly be puzzled to account for the assumption of $l_{r-1} - i_{r-1}$ and I_r, but the explanation is easy.

We know from the equation that $i_1 \quad o$, hence we can find i_2, then i_3, etc., to the last span. We also know from the equation that $I_s = l_s$, hence we can find I_{s-1}, then I_{s-2}, etc., back to the first span, and the operation is complete.

For constant moment of inertia

$$L_r = \frac{l_r}{\theta} \qquad G_r = T_r = \frac{2 l_r}{\theta}$$

where θ is six times the modulus of elasticity multiplied by the moment of inertia and is constant. Hence for this case we have

$$\overline{AS} = \frac{l_{r-1}}{3} = \overline{BC}$$

and

$$\overline{SC} = \frac{l_r}{3} = \overline{AB}$$

and the construction is simpler.

The next step is to use the definition of these fixed points to determine the moments at the supports. Now, A_r and B_r (see this *Journal*, January, 1891) are terms which depend on the load in the r^{th} span and for supports on a level and only this span loaded, we deduce

$$M_r = \frac{I_r i_r \frac{A_r}{L_r l_r} - i_r (l_r - I_r) \frac{B_r}{L_r l_r}}{I_r - i_r} \qquad (23)$$

$$M_{r+1} = \frac{(l_r - i_r)(l_r - I_r) \frac{B_r}{L_r l_r} - i_r (l_r - I_r) \frac{A_r}{L_r l_r}}{I_r - i_r} \qquad (24)$$

These equations are similar to those given in this *Journal* for November, 1889, except that we now have

$$\frac{A_r}{L_r l_r} \text{ and } \frac{B_r}{L_r l_r}$$

If we call the first one $= A_r'$ and the second $= B_r'$, they will be of the same form and for constant moment of inertia will have the value given there. Hence the diagram given in that article could be used, but the present one (*Fig. 2*) is simpler and less liable to graphical error.

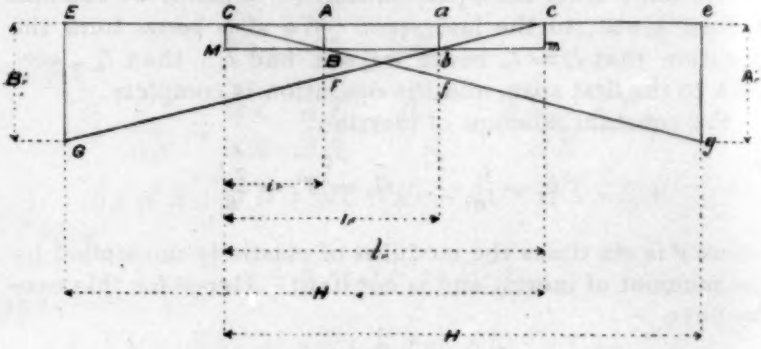


FIG. 2.

In this figure $\overline{Cc}$ is the span and $\overline{CA} = i_r$, $\overline{Ca} = I_r$ $\overline{EG} = B_r'$, $\overline{eg} = A_r'$. The latter two are laid off to a large scale. Also $\overline{Ec} = \overline{Ce} =$ is the length of a line that an arbitrary force H (usually the pole distance of the force polygon) would be, if constructed to the same scale that P (the load) is taken, in the graphical determination of A_r' and B_r'. It is better to have this scale considerably greater than is convenient for the force polygon or shear line. Since A_r', B_r' and H are laid off to the same scale, the tangents

$$\frac{A_r'}{H} \text{ and } \frac{B_r'}{H}$$

remain the same for any scale.

From the drawing we obtain the following:

$$\overline{ab} = \frac{B_r'}{H}(l_r - I_r)$$

$$\overline{af} = \frac{A_r}{H} I_r$$

and subtracting

$$\overline{b\,f} = \frac{A_r''}{H} I_r - \frac{B_r'}{H} l_r - I_r)$$

and therefore

$$\overline{C\,M} = \frac{\overline{b\,f} \cdot \overline{C\,A}}{\overline{A\,a}}$$

$$= \frac{I_r\, i_r \left(\frac{A_r'}{H} - \frac{B_r'}{H}\right) i_r (l_r - I_r)}{I_r - i_r} = \frac{M_r}{H}$$

$$\overline{A\,B} = \frac{A_r'}{H} i_r \qquad \overline{A\,F} = \frac{B_r'}{H} (l_r - i_r)$$

$$\overline{B\,F} = \frac{B_r'}{H} (l_r - i_r) - \frac{A_r'}{H} i_r$$

whence

$$\overline{c\,m} = \frac{\overline{B\,F}}{\overline{A\,a}} \cdot \overline{c\,a}$$

$$= \frac{\frac{B_r'}{H}(l_r - i_r)(l_r - I_r) - \frac{A_r'}{H} i_r (l_r - I_r)}{I_r - i_r} = \frac{M_{r+1}}{H}$$

The diagram required for the moments due to the sinking of the r^{th} support, a distance h below the level of the rest is the same as that given in this *Journal* for November, 1889, except that for variable moment of inertia we are to take (in that diagram)

$$\overline{D\,p} = \frac{h}{L_r\, l_r\, H}$$

which for constant moment of inertia reduces to

$$\frac{\theta\, h}{H\, l_r^2}$$

which is equivalent to the expression given in that article.

This diagram can be used for the sinking of every support, except the first and last. For these, formulæ which themselves indicate the simple diagrams required, would be convenient.

For the first support we want M_2 and Y_2 is the only Y that is not $= o$ in the original equations, and this has the value

$$\frac{h}{l_1}$$

and we, therefore, have

$$M_2 = \frac{d_2\, c_2\, Y_2}{Z_s} = \frac{d_2}{Z_s}\frac{h}{l_1}$$

Make $r = 1$ in equation (11*a*), and we have

$$Z_s = (c_1\, d_2 - c_2\, d_1)\, L_1 = -\, d_1\, L_1$$

whence

$$M_2 = -\frac{h}{L_1\, l_1}\frac{d_2}{d_1} = \frac{h}{L_1\, l_1}\frac{(l_1 - I_1)}{I_1}$$

In the same manner for the last or $(s+1)^{th}$ support we want M_s and Y_s, is the only Y that is not $= o$ in the original equations, and it has the value

$$\frac{h}{l_s}$$

and we, therefore, have

$$M_s = \frac{c_s\, d_s\, Y_s}{Z_s} = \frac{c_s}{Z_s}\frac{h}{l_s}$$

Make $r = s$ in equation (11*a*) and we have

$$Z_s = (c_s\, d_{s+1} - c_{s+1}\, d_s)\, L_s = -\, c_{s+1}\, L_s$$

whence

$$M_s = -\frac{c_s}{c_{s+1}}\frac{h}{L_s\, l_s} = \frac{h}{L_s\, l_s}\frac{i_s}{l_s - i_s}$$

The quantities

$$\frac{A_r}{L_r\, l_r} \text{ and } \frac{B_r}{L_r\, l_r}$$

for constant moment of inertia reduce to $P(2\,k - 3\,k^2 + k^3)$ and $P(k - k^3)$, respectively, where P is the load and k is the ratio of the distance of the load from the left support to the length of span. The graphical method of obtaining these quantities was given in this *Journal* for November, 1889.

The foregoing method of obtaining unknown quantities by fixed points may be called grapho-algebraic. That is, it may be applied to any problem in which the equations have this form. The modifications of formulæ necessary for coëfficients different in each equation will suggest themselves after a little study, but are not developed here for want of practical utility. The fixed points would be the same, but the expressions for them would be different. The diagram for obtaining moments would be unchanged if the conditions by which the V^s co-exist were the same as for the continuous girder.

THE ALUMINIUM PROBLEM.

BY JOSEPH W. RICHARDS,
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[*A lecture delivered before the Franklin Institute, January 12, 1891.*]

The lecturer was introduced by Prof. Persifor Frazer, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

Before beginning my lecture proper, I should say that shall take a liberty which a very few years ago would have been a presumption, but which to-day, and especially in this place, will, I do not doubt, be readily granted me. I refer to the fact that I am going to assume on the part of my audience some, perhaps considerable, familiarity with the general properties of this new metal. Within a year or so, two well-known Philadelphians have discoursed in this hall about aluminium, its appearance, remarkable properties, applications, etc., and I intend to repeat to-night very little of what has been said here before.

The title of my lecture is "The Aluminium Problem," and for the time being, taking these words as my text, we will first direct our attention to the word "aluminium," the metal of which our discourse treats. It may be interesting

to begin by inquiring how this word should be spelled and pronounced. You all know that two ways of spelling it are in common use, both Webster and Worcester sanctioning either way, Webster giving *aluminum* as preferable, and Worcester *aluminium;* you have also observed that I use the longer spelling. Let us inquire into the history of this matter.

Sir Humphry Davy was the first to give a name to the metallic basis of alumina, although it is not probable that he ever isolated the metal. He said, in 1808, that if he had been successful in decomposing alumina and isolating the metal in it, he would have suggested for it the name *alumium*. Davy evidently intended this word to represent "the metal from alum," simply taking *alum* and adding *ium*, as the proper termination for a metal. Objections were at once made to this name, not to the termination *ium*, which was considered absolutely proper, but to the root or stem of the word. It was maintained both in France and Germany that the name of the new metal should be derived from its oxide, and not from such a complex salt as alum, of which it happened to be an ingredient. The French name for the oxide was *alumine*, so they formed and have always used *aluminium*. The German name for clay was *Thon*, and for alumina *Thonerde*, so Gilbert suggested, in his famous *Annalen*, to call the metal *Thonerde-metall*, or, for short, *Thon-metall*. Fortunately, none of the German writers adopted this suggestion, but most of them adopted *alumium*. I find one instance of the metal being called, in Sweden, *argillium*, from the French word for clay, *argille*.

Whether Davy recognized the justice of these criticisms, or whether he considered that *alumium* needed an extra consonant simply for euphony's sake, we find him, in 1812, making bad worse by spelling the name *aluminum*. It is probable that he felt that the French were correct, but was unwilling to conform entirely to their mode of spelling. However, the name first proposed by Davy had taken root in Germany, and in 1812 we find the English using *aluminum*, the French *aluminium*, and the Germans *alumium*. The French have always used *aluminium*. The Germans used

alumium exclusively until 1824, when Wöhler, translating a paper of Berzelius from the Swedish used *aluminium*, which he also used in his famous memoir describing the isolation of the metal, in 1827, and which has been used in Germany almost exclusively ever since.

As to the English, they used *aluminum* altogether, until Wöhler's discovery, when the spelling favored by Wöhler was almost universally accepted, *aluminum* being used only now and then by some patriotic Englishman, averse to foreign innovations. We may say, therefore, that since 1827 the scientific world accepted and used the term *aluminium*.

Americans, however, have been most active in resurrecting the old English term *aluminum*. They say it is the shorter (true) and *ergo* the better way. That is what I should call intensely American. Mr. Eugene H. Cowles tells us that the Cowles Electric Smelting and Aluminum Company was originally organized as the Cowles Electric Smelting and Alumin(*i*)um Company, but that the writing of that extra *i*, and especially the dotting of it became such an insufferable burden and expense that they went to the court about it, and had the name changed to the Cowles Electric Smelting and *Aluminum* Company.

The question now is, which of these two forms shall we use? If *aluminium* is positively too long for practical, every-day use, and must needs be shortened, it is mere trifling to say that the dropping of one *i*, the removal of one letter out of nine, is any satisfactory means of accomplishing that end. But, if a shortening must be made, if manufacturers need it and public opinion demands it; if, above all, it would, in any way, save dollars and cents and lower the market price of the metal, then let us make a shortening which will be radical, which cannot be improved upon, and adopt Mr. Oberlin Smith's suggestion to call the metal *alium*. I think the suggestion a happy one, and if any change from *aluminium* should become necessary, I should be in favor of, and be one of the first to use, *alium*.

We will next consider what ground is intended to be covered by the word "Problem" in the title of my lecture.

There are many problems connected with aluminium. Primarily, of course, there is the problem of its production; next, that of its applications—casting, drawing, tempering, hardening, welding, polishing, turning, spinning, stamping. One very important question is soldering this metal. Then there is the problem of alloying it correctly and in proper proportions with other metals. The chemical analysis of aluminium is also a subject requiring more study than has yet been given it. Of all the problems connected with this new metal, I have singled out the one which is, par excellence, *the* aluminium problem, and that is, *the extraction of aluminium from the materials in which it is found in nature.* This is the one problem of paramount interest to the metallurgist.

Let me now divide my lecture into two parts, which may not appear at first sight to differ much from each other, but which are, indeed, very distinct. I wish to consider the aluminium problem as I have defined it under the two headings: (1) The isolation of aluminium. (2) The production of aluminium cheaply. The principle on which this division is made is not only logical but chronological; the first part intends to treat of the many efforts which were made to bring aluminium to view, from the earliest recorded experiments on the subject to the time when the first lump or pencil of aluminium was made by Deville, in 1854; the second part will include all subsequent methods.

We will inquire, in the first place, how the existence of the element aluminium became known to science. To do this we will start as far back as the times of the Romans, whose historian Pliny tells us that *alumen* was a name applied to several kinds of salts used in dyeing, a white kind for dyeing bright colors, and a dark for dull colors; that all alumen had an astringent taste and was colored black by the juice of the pomegranate. From all we can learn from Pliny and Columella, it seems that alumen was, therefore, a mixture of aluminium sulphate with more or less, always with some, sulphate of iron. Why it was called *alumen* is not certainly known. About the eighth century, we hear of a very pure *alumen* being made in Egypt and

afterwards in Rocca, near Smyrna, Asia, and hence called *alumen Roccæ* or *rock alum*. This was, indeed, a pure sulphate of alumina with sulphate of potash; in fact, our modern alum, differing, indeed, from the old *alumen*, but since it was used for the same purposes it was given the name of *alumen* or *alum*, and considered simply as being a very pure kind. This explains how our modern alum received its name.

The vitriols, sulphates of iron and copper, were also included under the term *alumen* or *alum* until about the twelfth century. Even in the thirteenth century, alum and vitriol were named together as related substances. The alchemist, Paracelsus, first strictly separated these salts, on the ground that the base of the vitriols was metallic and that of alum earthy. We understand, then, that in the beginning of the seventeenth century, alum was considered to consist of sulphuric acid and an earth whose nature was unknown.

Ettmüller, in 1684, in his *Chymia Rationalis ac Experimentalis*, says: "Alum is obtained by acting on clay with sulphuric acid." He had evidently discovered that the earthy base of alum is contained also in clay.

Stahl, in 1702, propagated the idea that this unknown earth was of the nature of lime or chalk, and for over fifty years most chemists followed this view.

In 1722, Hoffman announced that the earth of alum was of an entirely distinct nature from any other earth then known. Geoffroy in 1728, Hellot in 1739, and Pott, in his famous *Lithogeognosia*, in 1746, all reiterated this statement with increasing certainty until it was finally received as an accepted fact, and the last-named chemist gave it the name of *Thonichte Erde* or *terre argilleuse*.

In 1754, Marggraff, in three able dissertations on alum and its earth, showed that this earth was certainly a distinct substance; that it existed in all natural clays, can be extracted therefrom by sulphuric acid; that the part of the clay not touched by the acid is silica, and that, therefore, the purest white clay contains only silica and the earthy base of alum. These memoirs settled conclusively the

composition of clay, and gave rise to the term *argil* or *argil pûr* as the name of the earthy base of alum. It was thus called until 1762, when Morveau, intent on revising chemical nomenclature, decided that since alum was called *sel alumineux*, the proper name for its earthy base should be *alumine*, thus avoiding all confusion with *argille* and *terre argilleux*. The Germans, however, still call the earth of alum *Thonerde*, the French call it *alumine*, which the English also did until about 1820, when it was anglicized into *alumina*.

I left off with Marggraff in 1754, by whom the question of the composition of clay and the individuality of the base of alum was finally settled. Soon after this are to be found the earliest indications that alumina was suspected to be a compound body, and the search after aluminium began.

In 1758, Macquer wrote as follows: "The earth of alum is white, infusible, and since it changes color on being heated, I suspect it has some sort of a relation with the metallic earths." He was arguing from wrong premises but his conclusion was, by accident, not far wrong.

And now we come to the first recorded attempts to isolate aluminium. The experimenter's name was M. Baron, a professor of chemistry in Paris, who, in 1760, communicated a memoir to the Académie Royale, in which he says: "I believe the base of alum to be of a metallic nature for the following reasons: It has almost no properties in common with the known earths, while it has analogies with the metallic earths, such as changing color when heated, its astringent salts," etc. Then M. Baron tells us that he tried all known methods of reduction, but could not succeed in getting the metal. He concludes by saying: "If I had been fortunate enough to reduce the base of alum to a metal, no other argument would be needed, and that which to-day I conjecture would be a demonstrated fact. I am far from regarding the problem as an impossibility. I think it not too venturesome to predict that a day will come when the metallic nature of the base of alum will be incontestably proved." It is to be regretted that M. Baron does not record the manner in which he made his experiments, but

it is probable that they were mostly concerned with attempts to reduce alumina by mixing it with carbon or some organic substance, with some such additions as salt or black-flux, and heating as highly as possible in a coal or charcoal fire. Nevertheless, it is some satisfaction to know that he tried all the methods of reduction then known.

In 1782, Lavoisier, investigating the principles of combustion and oxidation, arrived at the conclusion that alumina was the oxide of a metal, the affinity of the metal for oxygen being so strong that carbon was not able to overcome it, and the oxide was irreducible by any hitherto known process. Lavoisier, moreover, succeeded in fusing alumina in a charcoal fire fed by pure oxygen.

In 1790, two Austrian chemists, Messrs. Ruprecht and Tondi, made experiments on reducing alumina. They thought that hitherto the reducing agent had not been intimately enough mixed with the alumina, and that the temperature used had not been sufficiently high, so they mixed alumina very intimately with charcoal dust, made into a paste with oil, and spread it on the inner walls of a Hessian crucible, the crucible being finally filled up with powdered charcoal, a layer of bone ash being placed on top. Putting the crucible in a forge, a strong fire was kept up for three hours. As a result, small metallic particles were found on the sides of the crucible. These they supposed to be the metallic base of alumina.

M. Savaresi in Italy, and Klaproth and Karsten in Germany, repeated this experiment and analyzed the globules of metal obtained. They found them to be phosphide of iron, the iron coming from the charcoal used, the phosphorus from the bone ash. Prof. Klaproth referred to this incident as "the pretended metallization of the earths," and said further, "If there exists an earth which has been put in conditions where its metallic nature should be disclosed, if it had such, an earth exposed to experiments suitable for reducing it, tested in the hottest fires by all sorts of methods, on a large as well as a small scale, that earth is certainly alumina, yet no one has yet perceived its metallization."

In 1807, Sir Humphry Davy, fresh from his victory over potassium and sodium by the aid of his great battery, attacked the alkaline earths in a similar manner. His first experiment, mixing alumina with red oxide of mercury, and passing the electric current through this in contact with mercury, was a total failure. Davy then began to think in earnest how he might reduce alumina. He remembered that potassa has a strong affinity for alumina, uniting with it easily, and that since the oxygen in each of these compounds could not be the cause of their affinity for each other, it must be attraction between the two metals. He then argued that if he first combined the two oxides and then put the compound under conditions in which potassium, if alone, would certainly come down, that in so doing it might possibly, because of its affinity for the metallic base of alumina, induce it to come down also. This was an idea worth trying, so he melted together one part of alumina with six parts of potash, in a platinum crucible, then made the crucible the positive pole of his battery and a platinum blade dipping into the fluid the negative. On passing the current from his battery of 500 plates, the result partially justified his expectations, for the blade soon received a metallic coating, and on removing it and dipping it into water the water was violently decomposed, the solution being found to contain alumina. An experiment with soda instead of potash gave a similar result. But the metal of alumina was always deposited with the alkaline metal, and Davy was unable to separate it by itself. In fact, when electrolyzing a compound of alumina with caustic alkali, both metals must neccessarily be set free if the compound is decomposed at all.

Davy's next idea was that it might be possible to reduce alumina by potassium itself, so he mixed some potassium with alumina, heated in a glass tube and observed that some potassium oxide was formed. This showed that some reduction had taken place, but he could see no metal of the earth. In the light of Winkler's recent experiments with alumina and magnesium vapor, we can probably conclude that some aluminium protoxide was formed.

A little later, in 1809, Davy had a new battery of 1,000 plates constructed, and experimented with this on alumina. He fused iron to whiteness in the arc, in contact with alumina, which became quite fluid at the temperature obtained. The iron became somewhat whiter, and when dissolved in acids showed that it contained aluminium. The fact was thus established, that *alumina can be decomposed, while fluid, in the electric arc, and its metal alloyed with iron.*

Davy was spurred to renewed endeavors. He placed alumina in a platinum tube and heated it to whiteness, meanwhile passing vapor of potassium over it—a most difficult experiment to perform. After cooling, mercury was introduced, in order to collect the metal if any had been isolated.

The mercury showed on solution only a trace of alumina, so that Davy was not sure if any alumina had really been reduced. The experimenter next mixed alumina with potassium and iron filings, hoping that if the potassium did reduce any alumina the iron might alloy with the metal. This experiment gave the best results obtained by Davy. On fusing the mixture a button resulted which was white and harder than iron, and undoubtedly consisted of an alloy of iron and aluminium.

Davy proved, then, that *fused alumina can be decomposed in the electric arc*, and that *alumina can be decomposed by potassium in the presence of iron*, but he was unable in any way to isolate the metal he sought for.

Prof. Benjamin Silliman went perhaps a step farther than Davy. Dr. Hare, of Philadelphia, had invented the oxy-hydrogen blow-pipe in 1802, and with it fused alumina to a milk-white enamel. In 1813, Prof. Silliman was repeating this experiment, supporting the alumina on charcoal, when he noticed small metallic globules rolling and darting out from under the fused mass and burning with a bright light. These globules could have been nothing else but *aluminium, reduced from fluid alumina by charcoal.* Thus another step was gained in the solution of the aluminium problem. The presence of oxygen at such a high temperature immediately oxidized these globules, and Prof. Silliman was unable to obtain the metal.

We now come to another advance, which was destined to lead ultimately to the solution of the aluminium problem as far as isolating the metal was concerned. The problem was attacked by a flank movement. Alumina could not be satisfactorily decomposed, but Oersted, in 1824, discovered a way to make a new salt of aluminium, its anhydrous chloride, Al_2Cl_6, and this opened the way to Wöhler's success three years later. Previously to 1824, aluminium chloride was known only in solution, as when alumina was dissolved in muriatic acid; but, on evaporating this solution, hydrochloric acid fumes pass off and alumina remains. Oersted passed chlorine gas over red-hot alumina, mixed with charcoal, contained in a porcelain tube. Anhydrous aluminium chloride distilled over into a condenser. It then occurred to him that perhaps potassium would reduce this new salt. The idea was right, but he made the mistake of using potassium amalgam instead of pure potassium, thinking that he might thus get an amalgam with aluminium and distil the mercury away from it. Potassium when amalgamated with mercury, however, is not powerful enough to reduce aluminium chloride, and Oersted missed, by a very little, the honor of first isolating aluminium.

Berzelius, the Swedish chemist, was a constant worker at the aluminium problem. He wrote to Sir Humphry Davy, in 1809, that he had reduced alumina by heating it with carbon in the presence of iron, but it is probable that he was mistaken, because he never refers to this in any of his later writings. However, after many attempts and repeated failures, he once came within an ace of solving the problem. He had studied carefully the mineral cryolite ($Al_2F_6.6NaF$) and determined its composition and chemical formula. He made it artificially, and also the corresponding potassium salt. It then occurred to him that this latter salt might be reduced by metallic potassium. He made the experiment in a crucible, and carefully washed the fusion with water, but found no metal. It is quite evident that his sole mistake was in using an excess of potassium, which, after the reduction, gave on solution in water a caustic alkali solution

which immediately dissolved all the reduced aluminium. If he had used an excess of fluoride salt the story would almost certainly have read differently, because this reduction has since then been repeatedly performed. The date of this experiment was 1825.

In 1827, Wöhler, the German chemist, after repeating Oersted's experiment with unsatisfactory results, ventured to modify the experiment by using pure potassium instead of its amalgam. He placed a lump of potassium in the bottom of a platinum crucible, covered it with an excess of aluminium chloride, and fastened on a cover. He then heated the crucible gently, waited until the reaction was over, and treated the resulting fused mass with water. *He obtained a gray, metallic powder, which was finely-divided aluminium.* This was the first real solution of the aluminium problem. Wöhler could not get this powder to gather into a button, in order to determine the properties of the metal *en masse*, but he found out many of its chemical properties, such as its resistance to acids, solubility in alkalies, etc., and produced from the metal some of its compounds, such as the sulphide and arsenide, which had not been known before. His observations were vitiated, however, by the fact that the powder contained platinum, which decreased its fusibility, and some potassium, which rendered it less resistant to oxidation.

At this point an unexpected obstacle arose to further advance. This powder could not be in any way united to a button. I have no doubt that Wöhler and others tried every conceivable way of doing this, but for many years without success. This was, to say the least, vexatious.

The question remained in this state until 1845, when Professor Liebig tried whether the reduction would proceed any easier if vapor of aluminium chloride were passed over potassium. He found this arrangement much preferable to Wöhler's old manner of working, but still nothing was obtained but the gray powder. Wöhler was still able to work at the subject, and he tried passing potassium vapor over aluminium chloride, with the long-sought-for result of *obtaining some globules of aluminium.* These were not much

larger than pin heads, while the powder accompanying them was no longer in scales, but was shown by the microscope to consist of numerous fine globules. This was encouraging, and Wöhler was able to determine that the metal was malleable and to get its specific gravity quite closely. But, just here, he made a fatal error. He picked out the largest globules to try to melt them into one large button, but used for a flux borax, not knowing that it attacks the metal strongly. Besides, the melted borax being of higher density than aluminium, the globules remained separated on its surface, where the oxidizing influence of the air kept them from uniting. The result was that Wöhler saw the globules get smaller and finally disappear, and hence concluded that they had oxidized away, and that aluminium was not a very stable metal after all.

The satisfactory solution of the aluminium problem was thus delayed until 1854, when Henri St. Claire Deville took up the question. He says that he did not know of Wöhler's results of 1845, and was repeating Wöhler's first experiments, always getting the gray powder. On modifying the experiment as Wöhler had done, he obtained globules similar to Wöhler's later results. This success encouraged him to renewed efforts to get the metal in mass. He found that metallic sodium was much safer to work with than potassium, performed the reduction just as well, and, further, enabled him *to collect the metal into a large button.* The reason for this latter result was that when the sodium acted on the aluminium chloride, the sodium chloride formed combined with the excess of aluminium chloride to form a double chloride, $Al_2Cl_6.2NaCl$, which was very fusible and acted perfectly as a flux to unite the metal to a button, being lower in density than aluminium and apparently dissolving away the coating on the globules which prevented them from running together. Further, since he operated in a glass or earthenware retort, the metal was free from platinum; he also obtained it free from sodium, and almost chemically pure.

We have now followed the aluminium problem through its purely experimental stage. We have passed in review

a history of ninety-four years of recorded experiments, from Baron, in 1760, to Deville, in 1854, and the first part of our problem is solved. Aluminium, *en masse*, can be isolated from its compounds.

By a most natural transition we pass directly to the second part of the aluminium problem, as I stated it in beginning my lecture, namely, "The production of aluminium cheaply." This problem is almost radically different from the first. During all those ninety-four years, expense was never once mentioned as entering in any way into the problem, but now the question is changed. We may conceive the fair goddess who presides over these secrets of nature to have said to Deville, "You have solved my first problem; you can show me a piece of aluminium?" "Yes" "Then I give you a harder one: produce it cheaply."

As Wöhler was the principal factor in solving the first problem, so was Deville the prominent figure in the solution of the second. His task was well-nigh herculean. I suppose that the first half-ounce lump of aluminium which he produced cost $50. He nobly bent all his energies to the work before him.

Before reviewing the work of Deville and other investigators since 1854, let us look for the real reason for all this serious work. What is the great fact that underlies the search after cheap aluminium; what is the main-spring of the aluminium industry? It is not that aluminium is the peer of all metals, for gold is more beautiful, platinum more stable, iron more useful; but it is the fact discovered by Deville that aluminium is in many respects a beautiful, durable, useful metal, coupled with the other great fact, known for a century, that next to oxygen and silicon it is the most abundant element to be found on the earth.

It is little wonder, then, that not only Deville but also chemists in all countries, of all nationalities, have spent time, money and brains without stint to advance the problem of its extraction. It is also little wonder that so many capitalists interested in the subject have often been induced to put their money into the hands of charlatans

and frauds, for the ostensible purpose of working at the aluminium question. It is not at all to be wondered at that this question has become one of the most noted metallurgical questions of our day, and its satisfactory solution will, I am sure, crown the last decade of this wonderful century.

The problem must now be considered on a commercial as well as a metallurgical basis. The question is, "What is the cheapest raw material to use, and what is the cheapest way of extracting the aluminium?"

Of all the natural aluminium compounds, clay is probably the most widely distributed. White china clay, or kaolin, is the purest, containing about equal parts of alumina and silica, or about twenty-five per cent. of aluminium. Colored clays contain less alumina and varying quantities of such impurities as iron oxide, lime, etc. Kaolin is abundant and cheap enough, and many of my audience have perhaps wondered why aluminium is not at present extracted from it. The reason is that by any of the methods used for producing aluminium, any silica present with the aluminium compound being reduced is invariably reduced to silicon, which alloys with the aluminium and injures very much its commercial value, a large percentage making it entirely unfit for use. It may then be asked, "Cannot the clay be first treated in such a way as to remove the silica?" True, it can, but to do this costs money, and the alumina thus obtained would be more expensive than that produced from other natural minerals. (I hope that this explanation will be satisfactory to those of my audience who have been in the habit of thinking of aluminium as "the metal made from clay." About a week ago I met a gentleman in the south, a colonel, of course, who, when I replied that aluminium was not being made from clay, remarked, "What, not being made from clay! Why, that takes all the poetry out of it," and his interest in the subject was so far gone that he changed the conversation.)

The two minerals which serve the aluminium industry for raw materials are cryolite and beauxite. Cryolite is a double fluoride of aluminium and sodium, $Al_2F_6.6NaF$, containing nearly thirteen per cent. of aluminium, while beauxite is an impure hydrated alumina containing from twenty-five to

thirty-five per cent. of aluminium. Cryolite is only found in quantity in Greenland, the mines are monopolized, and although costing only about $20 a ton to land in this country, the importers charge purchasers several times that price for it. Cryolite also contains some silica finely disseminated through it, which much impairs its usefulness for producing pure aluminium. Beauxite is really the most suitable mineral for furnishing alumina. It occurs abundantly in deposits in the south of France, Austria, Ireland and in Georgia. Its cost is from $6 to $10 a ton. Average specimens of the Georgia mineral contain one to two per cent. of silica and one and one-half to three per cent. of ferric oxide. With such a mineral to be bought at the price named, it is the cheapest source of alumina that the aluminium industry can find.

Returning to Deville, we will follow him in his task. About the first improvement he made was to reduce the cost of metallic sodium. He erected apparatus on a large scale, and reduced its cost from $100 a pound in 1855 to $1 a pound in 1859. Since it takes about three pounds of sodium to produce one of aluminium, the saving thus effected was a large proportion of the cost of the aluminium. His next step was to replace aluminium chloride by the aluminium-sodium double chloride. He found that this latter salt was less apt to deteriorate in the air, less volatile, more fusible and altogether much more satisfactory to work with, while it could be produced with even less trouble than aluminium chloride. A further improvement was to operate the reduction on a large scale, on the bed of a reverberatory furnace. Putting all these economies together, Deville reduced the selling price of aluminium in 1862 to $12 a pound, at which figure it stayed until 1887; that is, for twenty-five years. This shows the net result of Deville's labors, but it gives no idea of the hundreds of experiments which he made during those years. He tried electrical methods (of which I shall speak presently), the use of cryolite; he tried to produce sodium vapor from one retort and aluminium chloride vapor from another, and mix them in a third retort or reduction chamber. In fact, although his

works earned considerable money, yet he spent it almost all in experiments, and in 1859 he crowned his work by writing his book, *De l'Aluminium*, which is one of the most admirable treatises known to science.

Deville's labors and the process which he developed have been very ably supplemented by the work of Mr. Castner, who, by a very ingenious invention, reduced the cost of sodium to about twenty-five cents a pound, and by establishing the manufacture of aluminium-sodium double chloride on a large scale, lowered its cost about three-fourths. The result was that the works managed by Mr. Castner sold aluminium in 1888 at twenty shillings a pound, and later as low as ten shillings, but their process is now equalled in cheapness by the recent electric processes, and the fact is now apparent that the sodium processes for producing aluminium are being worsted by electricity, and their description will soon become a matter of history.

A few words only are necessary to describe the reduction of cryolite by sodium. You remember that Berzelius tried, in 1824, to reduce it by potassium, and was unsuccessful, because he used an excess of the reducing agent and washed the fused mass with water. When Deville made known his method of working on aluminium chloride, in 1855, Dr. Percy, in England, tried to produce aluminium by reducing cryolite in a somewhat similar manner, and was successful in getting the metal. H. Rose, of Berlin, also made similar experiments. The reduction was effected in iron crucibles, because cryolite corroded common crucibles, and the silica dissolved contaminated the metal with silicon. On the other hand, Rose's metal was often ferruginous. Deville gave this subject considerable attention, but decided that the return of metal was small, the metal obtained was impure, and, above all, the supply of cryolite was precarious and would not serve as the basis of a large industry. However, the Tissier Brothers built a works near Rouen, France, in 1857, based on the use of cryolite and its reduction by sodium. Their best metal was always siliceous, and they were in operation only about five years. Of late years, the cryolite method has been revived by the Alliance Alumin-

ium Company, which owns Dr. Netto's process for manufacturing sodium, by which this metal can be made nearly as cheaply as by Mr. Castner's process. In fact, they are selling sodium cheaper than the Aluminium Company, Limited, owning Castner's patents. The method adopted by this company was to melt a large quantity of cryolite, pour it into a ladle, and by means of an inverted cup force to the bottom of the ladle a large lump of sodium. The operation depended for its success on the large scale on which it was operated. This company competed for some time with the other sodium processes, but it is now eclipsed by the electrolytic methods.

We now come to the consideration of a part of my subject, which is essentially very interesting—the application of electricity to solving the aluminium problem. We have noticed its application by Davy, in 1807 and 1810, with only partial success. We will take it for granted that probably before Davy's time, and certainly between 1810 and 1855, the galvanic deposition of aluminium from aqueous solutions of its salts had been often attempted and without success. I may say here that since this mode of electrodeposition is, even when successfully operated, only suitable for producing thin coatings of aluminium, and not to producing aluminium *en masse*, I shall leave it out of further consideration this evening.

Somewhere about 1850, Prof. Bunsen made a new departure in electrolytic science by using as an electrolyte a fused bath of magnesium chloride and thus produced magnesium electrolytically. When Deville set about the aluminium problem in earnest, in 1855, he thought at first that the sodium process was not capable of much cheapening, and he cast about for some other method of production. Knowing of Bunsen's results with magnesium, he thought it possible that a fused bath of aluminium-sodium double chloride might answer as an electrolyte, aluminium chloride alone not being suitable because its melting point and volatilizing point are so close together. He tried the experiment, found that it would succeed, and in this way produced more aluminium than up to that time he had

made by the sodium process. The operation, however, was beset with difficulties. Carbon electrodes were used, and these disintegrated so quickly as to interfere seriously with the electrolysis. Bunsen was working at the same time in a similar manner, and the two investigators published their results almost simultaneously.

These experiments were really successful, but they were not brought into practical use at that time because the only source of the electric current at their disposal was the galvanic battery, whose costliness put a stop to all hopes of commercial usefulness. As soon, however, as magneto- and dynamo-electric machines placed at command large electric currents at small cost, these old experiments were quickly put into practice. Berthaut, in 1879, proposed the use of dynamo-electric machines in producing aluminium, and Grätzel, in 1883, made their first commercial application. Since then, numerous processes of this kind have been proposed and put in operation, differing from each other principally in the composition of the electrolyte used and the arrangement of the electrodes.

The conditions to be fulfilled in electrolyzing fused aluminium compounds are that the salt shall conduct electricity and that the current be of sufficient tension to decompose it; and then, in order that the aluminium liberated be practically obtained, there must be no opportunity given for a re-combination of the separated elements, and the bath must have no solvent action on the liberated aluminium. We will see how these details have been arranged in the different electrolytic processes.

Gaudin, in 1869, proposed the use of an electrolyte composed of equal parts of cryolite and sodium chloride. The advantage of this mixture was that it melted at a low temperature and was very fluid, its disadvantage was that at the high potential required to decompose the aluminium fluoride, the sodium chloride present was also decomposed and much sodium vapor liberated.

Kagensbusch, in 1872, proposed to melt clay with sodium chloride, and electrolyze, a metal being present to alloy with the aluminium as it was liberated. The trouble here is, of

course, the silicon, which is reduced along with the aluminium and renders the product worthless.

Grätzel used a dynamo-electric current, the electrolyte being cryolite or aluminium-sodium chloride, while the pot containing the electrolyte was of aluminium and made the negative electrode, and the positive electrode was a mixture of alumina and carbon pressed into shape. The particular advantages of this process were that the metal collected on the walls of the pot, while the chlorine or fluorine liberated at the positive pole was in immediate contact with alumina, on which it was supposed to react and thus regenerate the bath. A great deal of aluminium was made by the company working this process, but they state that the Grätzel process, as originally patented, was a failure, principally because of the disintegration of the positive electrode and the consequent clogging of the bath. The improvements which they have made to this process have not been made public.

Dr. Kleiner, of Zurich, used as an electrolyte pure cryolite, the electrodes being two pencils of gas-retort carbon, but he overcame some of the difficulties of the electrolysis in a very ingenious manner. The chief difficulty in working with cryolite is to get material for the vessel which contains it which will not be corroded by the molten salt. Dr. Kleiner filled a crucible with cryolite and imbedded in the middle his two electrodes. The current being turned on, these carbons were drawn far enough apart to form an electric arc, which very quickly melted the cryolite for a space of several inches around it. Part of the energy of the current was thus converted into heat to keep the electrolyte fluid, while the current also operated its decomposition. The process was not successfully operated, the chief difficulties being the corrosion and disintegration of the carbons and the accumulation of sodium fluoride in the bath, increasing very much its electrical resistance. However, the device of keeping the electrolyte fluid by part of the energy of the electric current has many advantages, and although it is rather a costly method of obtaining heat where steam-power is used for the dynamos, yet it is not

very costly where water-power is used, and it is an improvement which is being used in all the present successful electrolytic processes.

Hèroult, Hall and Bernard Brothers have patents covering methods of working somewhat similar to each other. They all electrolyze cryolite or an electrolyte of analogous composition, the bath being kept continually saturated with alumina. The effect of passing the current is in the end to separate aluminium at the negative pole and oxygen at the positive, and the principal difference between these processes is in the different explanations given as to the reactions taking place in the bath. I am informed that the Cowles Electric Smelting and Aluminum Company have also purchased an English patent (Bognski's process) which covers the same principle, and intend working for pure aluminium. The different explanations referred to are as follows: Mr. Hall claims that he decomposes alumina mechanically dissolved in the fused solvent, while other parties claim that the aluminium fluoride is the compound decomposed, but that the fluorine set free immediately acts on the alumina present to regenerate aluminium fluoride and liberate oxygen. Which of these two explanations is correct is not certain. I believe our respected secretary, Dr. Wahl, is on record as thinking the latter explanation more probably correct, while, from a recent inspection of one of the processes, I have been inclined towards the opposite conclusion; so, while the *doctors* differ, the *patient* audience may be left to take its choice.

If asked my opinion of the economy of these processes, I should reply that, metallurgically, they can be brought very near to perfection. Since the solvent remains unaltered by the current, aluminium is practically obtained direct from alumina, the cheapest source of aluminium, in one operation, while, since there is no avenue of escape for anything, no slag, no waste products, practically all the aluminium in the alumina is obtained. The two chief items of expense peculiar to the nature of these processes are, then, the cost of power and plant. With cheap water-power, which can be obtained in many places, the item of power will be reduced very low

—so low that it would no more than balance the expenditure for fuel by any other possible means of reduction. The other item of expense, however, is a standing one. Electrolysis is, primarily, a slow process; immense currents are needed for only moderate outputs; expensive turbines and still more expensive dynamos must be purchased and kept in repair, while, finally, the number of pots needed for only moderate outputs renders the plant expensive, and interest on this capital will always form a large item of the expense of these processes.

There are two electrical processes which I have purposely not mentioned among the electrolytic processes, and I now discuss them here together: Cowles Brothers' and Hèroult's processes for producing aluminium alloys. The principle involved is the interruption of a powerful electric current, the formation of an immense arc, and the reduction in this arc of alumina, by carbon, in the presence of a metal. The working of both of these processes is too well known to require my tarrying to describe them to-night. In Cowles Brothers' furnace the current is interrupted by having to pass through a box filled with broken pieces of carbon, the whole interior is raised to a very high temperature, at which the alumina present is reduced by the carbon and alloys with the metal. That this is the rationale of the process there can be almost no question. The Hèroult apparatus is arranged differently. Alumina is fused by the electric arc, and floating on molten copper, is then treated as though it were an electrolyte, a carbon rod dipping in the alumina being the positive pole and the copper the negative. The patentee of this process claims that it is essentially electrolytic, and indeed it may be allowed that some electrolytic action may take place, because the parts are arranged in just the correct way to obtain that result, but, by a quantitative study of the quantity of the electric current and the amount of metal reduced, both in this furnace and in the Cowles process, it can be shown that several times as much aluminium is produced as the current could be able to separate electrolytically. It is therefore evident that the larger part of the reduction in these furnaces is due to some other agency than electrolytic

action, and this other agency can only be the reducing action of carbon. Of the two furnaces, I think that the Hèroult is the better arranged and in best shape for practical working.

From the preceding observations, we may then consider the fact established that *alumina can be reduced by carbon*, at a temperature somewhere not far above the fusing point of alumina, which is below 2,600° C. This opens up the possibility of simpler processes of reduction than any yet known. Is it because the alumina is fluid that carbon reduces it, or is it solely because of the high temperature? I cannot but think that the fluidity of the alumina plays an important part in this reduction. But, may it not be possible to render alumina fluid at a lower temperature than now appears necessary? I think that it may. For instance, when I dissolve iodine in alcohol, rubber in carbon bisulphide, do not these substances become fluid at ordinary temperatures? Cryolite and some other salts appear to dissolve alumina mechanically at about a red heat, and if this action be purely mechanical, then we certainly have alumina fluid at that heat. I think it quite probable that alumina thus rendered fluid could be reduced by carbon at a much lower temperature than is required to do it in the electric furnaces.

You will have gathered from these remarks that I am by no means wedded to the idea that we will find the final solution of the aluminium problem in electrical processes. I consider that they are handicapped by an expensive plant, and although electrical processes are "on top" at present, and I think will stay there for some time to come, yet I am far from considering that they may not possibly be superseded by non-electric methods of reduction. I do not consider the sodium processes, however, as ever likely to take part again in the metallurgy of aluminium. I think their day is past, but I now refer to methods of reduction by common reducing agents. Those who have read the literature relating to this subject must have concluded that almost every possible or impossible means of reduction has been tried, with but slender success. Calvert & Johnson produced iron-aluminium alloys with as much as twenty-

four per cent. of aluminium by the reducing action of iron on aluminium chloride. A similar alloy with three and one-half per cent. of aluminium was made by Faraday & Stodart, by the reducing action of highly-carburetted iron on alumina. Pig irons have been analyzed containing as much as one per cent. of aluminium. Zinc will under some circumstances reduce small amounts of aluminium chloride and cryolite. Half a dozen of the common metals can reduce aluminium sulphide, among which tin seems to do it easiest and most completely. Under peculiar conditions zinc will overcome the affinity of aluminium for oxygen. We have in several places come across the fact that carbon reduces alumina at a temperature elevated enough to fuse the alumina. So, while ordinary reducing agents have been tried in almost every conceivable way, I hope and believe that they will yet come to the front as important factors in producing cheap aluminium.

Gathering all the threads together, catching the drift of all that has been touched upon in this rather rambling discourse, the general conclusion is, I think, that progress, very satisfactory progress, has been and is being made. Rome was not built in a day, and I, for one, am quite satisfied with the progress of the aluminium industry. Six years ago, aluminium sold for $12 a pound, three years ago for $5, to-day it is being sold in England at $1.50, and before this year is out it will probably be down to $1. Aluminium was never before sold as cheaply as it is now. The prospects for cheaper aluminium were never more promising than they are to-day.

ELECTRICITY AS THE RIVAL OF STEAM.

BY DR. LOUIS BELL.

[*A lecture delivered before the Franklin Institute, January 9, 1891.*]

The lecturer was introduced by Prof. Edwin J. Houston, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

It is not my purpose here, to discuss in learned phrase electrical science and electrical transmission of energy, nor to lead you into complicated studies of the practical details of the ways in which we employ electrical energy in daily life. I desire simply to put before you a common-sense view of some of the possibilities and limitations that present themselves when we try to make use of the potent force that is to-day becoming so important a factor in our modern civilization. But before discussing the properties of electrical energy, either for their own sake or for comparison with other things, let us delay for a little and attempt to establish in our minds some sort of definite idea as to what manner of thing electrical energy is, what its most prominent properties are, and how it is connected with the forces that are more familiar to us. We are to-day in a transition stage as regards our view of electrical action. Not so very many years ago scientific men were content to think of what they called electricity as a subtle fluid, to use the time-worn phrase, plastered, as it were, upon material things by certain processes, but with the extraordinary peculiarity that in applying one plaster another similar in some respects but very dissimilar in others, invariably appeared on objects in the vicinity. In other words, electricity was looked on as a fluid that in some way was a mysterious attribute of the surfaces of bodies and produced remarkable effects on neighboring bodies; wherever a charge of the so-called positive fluid appeared another closely similar equal charge of hypothetical negative fluid must appear on bodies

near by. The electric current was looked upon as a flow of this extraordinary fluid through and along a wire much as water flows through a tube; and magnetism by a barbarous sort of analogy was imagined to be composed of two other extraordinary fluids plastered upon the surface of iron and steel bars, and exerting marvellous pulls and pushes on other magnetic porous plasters in the neighborhood. I am going to ask you to-night to turn away from all this and look at the same fundamental facts from a totally different direction.

Now, in the first place, the thing with which we really have to do in every case of electrical action is not the indefinite something called electricity, but a very definite something—electrical energy. We have energy turning up in a great many different forms all about us, as heat, the energy of a falling body, the energy of something thrown, and in almost innumerable other ways. In the case of the energy of a cannon ball, for example, we find it can be measured by the product of the mass of the ball into half the square of its velocity. In looking at almost any other manifestation of energy we find that we can conveniently divide it into two factors, such as mass, velocity; force, and the distance through which it acts; and so on. Now, electrical energy into which we can at will transform the mechanical energy supplied by a steam engine is made up in a precisely similar way of two factors; one of these is potential, the other electricity. For most purposes we may think of electricity, whatever it is, as being only a coëfficient of the potential, by which the latter is connected with electrical energy. What the physical meaning of this coëfficient may be, in other words what electricity is, we are not prepared to state. It certainly, however, is not electrical energy itself, but only a part of it. Now, experimentally, in every case we are dealing with electrical energy, for we can hardly imagine such a thing as an electrical charge without a potential. If it really exists we have a very small chance of ever becoming acquainted with it, since we can only experiment by means of apparatus that requires a certain amount of work to be done upon it in order to show

any indications whatever. Suppose we produce electrification upon any body by any means we choose, we shall find that the electrified mass will produce a series of pushes and pulls upon light bodies in its vicinity and all about it; in fact, throughout all the region without the body in question we find that there are a series of stresses, minute pulls and pushes, tending to act upon other bodies, and that the whole region is in a state of strain that can readily be made to show itself; a certain amount of energy has been taken from something to set up this effect, which really is potential electrical energy. Now, if we hunt around through the strained region about our electrified body we shall find that every piece of matter in the vicinity has been affected by the stresses that exist, and that its surface is, like the surface of the original body, electrified. Just what goes on at the surfaces themselves is of comparatively little moment, the interesting fact being that the whole region between them is in a state of strain from the electrical energy that has been furnished to it. We should, therefore, think of an electrified body not as having a surface charge of something about which we are totally ignorant, but as the centre of a system of stresses and strains; by stresses I mean forces acting, pushes, or pulls, or twists, or spins; and by strains the displacements, the effect in whatever is subjected to them.

So far as electrification goes, then, it is perhaps sufficient if we think of it as the manifestation of electrical energy that is found at the surface between media having different dynamical properties; just as we find a certain set of phenomena—reflection, refraction, polarization, and the like, at the surfaces between two media that have different dynamical properties regarding light. The familiar fact that electrification is only on the surface of the conductor, follows naturally from this view of the subject, and is only another way of saying that the stresses and strains produced by electrical energy change their character with the substance that is subject to them. Thinking, then, of electrification as merely a surface condition of the electrical energy acting through all the space outside of the surface, we can

easily see the reason for some observed facts in electrical science. What we call lines of electrical force are merely lines which mark the direction of the stresses in the medium surrounding an electrified body. Where these lines of force, that is stresses, act upon matter they produce some sort of strain in it of the kind that we recognize by its effects as electrification. It is quite needless to state that these lines of stress must either terminate in matter, or close upon themselves; for one can hardly imagine such a thing as a strain with only one end, and if we can suppose the sort of strain that is produced by electrification—that is, the communication of electrical energy—to have a certain kind of endedness, as it were, it is not difficult to imagine the reason why the electrification at one end of the line of force is different in quality, but the same in quantity as that at the other end. So we may sum up by thinking of electrification not as the production of a film of electricity on the surface of a body, but as the system of stresses and strains produced around a material body by expending electrical energy upon it. The lines of stress pass outward from the surface of the electrified body to other bodies, in which they produce electrification. In all this there is nothing revolutionary, we are merely looking at the two surface electrifications as terminating a stress in the medium between them.

Now looking at a charge of electricity merely as a centre of stress exhibited in the medium outside, what will happen when that centre moves, as, for example, along a wire? Evidently the character of the stresses in the medium will be somewhat changed, depending upon the exact way in which the movement takes place. Now, as a matter of experiment, we find that when there is such a movement of the centre of electrical stress, as when what we call an electro-motive force is free to act along a conductor, the result is a new set of stresses and strains corresponding to a spin of the medium about the direction of motion as an axis. This spin in the medium is a kind of kinetic energy, the particular kind we call electro-magnetic; but if this view is correct, if we can produce electro-magnetic stresses about a wire by letting a

charge flow along it, it would seem probable that we ought to be able to produce a similar effect by moving the charged body rapidly through space without any conduction whatever. Such is the case, as was shown by some beautiful experiments of Prof. Rowland's about fifteen years ago. Now there is one curious case that can occur with these electro-magnetic spins about the wire; suppose the electromotive force to be an oscillatory one, so that the charge, the centre of stress, will pass rapidly from one end of the wire to the other and back. You can see at once that the result would have to be a pulsating, wave-like strain in the medium, composed still of electro-magnetic spins, but intermittent instead of continuous ones. These pulsations, waves of energy given off from the conductor, may be recognized by their effects as waves of electro-magnetic energy. Like every other sort of strain, push or pull, or whatever it may be, these waves will move out from their starting point with a certain velocity depending upon the medium through which they are passing; in an enormously elastic medium the velocity will be very great, while in a dead, viscous, substance the impulses will be transmitted but slowly. Now, as there will be a certain number of these pulses of energy per second, depending upon the rapidity of the oscillations that caused them, and as they travel at a certain velocity, never mind what it is, each electro-magnetic wave may be said to have a definite length; in other words, each pulse will have travelled a definite distance before the next one starts. Now, it is a very marvellous discovery, suspected twenty years ago, but only proved two or three years ago, that when these waves of electro-magnetic energy are sent out with such rapidity that when the length of each is only about the fifty-thousandth part of an inch, they come within the ken of our senses, and we have all been very familiar with them by their old name of light. If they become a little longer we feel them as radiant heat, and finally we come to such as may be produced by mechanical means, as in an alternating dynamo, where the waves would be something like 2,000 miles long. So we must recognize in light and radiant heat only forms of electro-magnetic energy; or,

rather, we find that they are the same kind of energy that we have been wont to call electro-magnetic.

Suppose, however, that we have no waves, but only steady spins in the medium around the conductor, such as may be set up by the flow of electrostatic stresses, that I have described. The lines of force are then in circles around the wire, and if we coil up such a wire you will see that these circles interlock with the different coils, and we have a set of lines of force shooting through its centre. But such a coil of wire is a magnet, and the addition of an iron core only stren-gth ens, but does not change the character of the stress produced in the medium about it to any appreciable extent. So you see, starting simply with the system of stresses about a material that exhibits the kind of surface strain we associate with electrification, we may come quite easily to the idea of lines of force, having definite ends and producing electrification in other bodies; or, if we imagine the electrification to travel along a wire, it may produce electro-magnetic waves, or steady electro-magnetic force, which can be easily connected to the familiar form of magnetism. Meanwhile, we have scarcely mentioned electricity as such, in fact inasmuch as we cannot experiment on it, since it is not electrical energy, but only a coëfficient of it, we can form only very indistinct ideas as to its real character. We know only that what we call an electric charge produces, or accompanies, or *is* a superficial strain in mattter, which becomes the centre of a system of stresses in the region outside of it, in which we find much to interest us. The important question is how to produce such a condition of things, for we know by experience that it has very useful properties, as may be seen in the manifold applications of electricity of which I shall presently speak. In theory we can produce mechanical stresses that give an almost identical distribution of energy around the centre of disturbance; for example, if we had a tube open at both ends and immersed in a fluid, and then could suddenly start the fluid in this tube into motion, the lines of flow about the tube would correspond almost exactly with the arrangement of stresses about a bar magnet. A mass of jelly, distorted by a twist applied to a rigid bar imbedded

in it, gives the same distribution of stress that is found in certain cases of magnetism. In similar fashion the electrical stresses may be imitated, so that at all events we need not have to imagine any enormously complicated mechanism for producing the state of things we find. Unfortunately until we know very much more than at present about the intimate properties of matter we hardly know where we should begin work upon it. However, both electrostatic and magnetic action can be very readily produced; in fact, it is very difficult to imagine any operation that can be performed on matter without transmuting energy into one or the other of the forms just mentioned.

In general, however, there are only two convenient ways of getting electrical energy in quantity. By acting chemically on the surfaces of certain metals we can start up, as it were, the kind of stress we call electro-motive force, and get electricity flowing along a wire, but the more convenient method is to start with a magnet; all around it, as we have seen, there are stresses and strains in the medium, and the region where they exist we call a magnetic field. We have said thus far very little about the medium concerned, but we know that it is not air for a vacuum is no obstacle to magnetism, and we know that it permeates all sorts of matter. From the identity of electro-magnetic energy with light we know that the medium in question is what is often spoken of as luminiferous ether. Now, if we push a wire through this region of strained ether about a magnet we find, as might be expected, that the stresses react on the wire, and they actually set up that state of things which we call electro-motive force, hence a current flows in the wire and the energy required to maintain it we have to furnish, and therefore the wire moves a little harder through the magnetic field than outside of it. Now, a dynamo is nothing in the world but a machine for dragging loops of wire through a magnetic field, and thereby setting up currents in the wires. The energy that appears in the current comes from the power that is pulling around the wires. It is just as when one drags a paddle wheel through still water, holding its axle lightly so that it may turn; the wheel can do

work in turning but you have to furnish the power by pulling it along—there is no energy acquired from the water. Conversely, if you pass an electric current through a wire that is in a magnetic field, and thereby set up spins in the medium about the wire these will react on the stresses already in the medium and tend to fling the wire out of the field into a region where no stresses exist; this would correspond, in the hydraulic case we have just mentioned, with a paddle wheel floating in water and turned by hand; it will move along and push things before it, but the power has to be supplied externally. Here we have the principle of the electric motor, which you see is nothing but a dynamo working backwards. In either machine the important things are not the wires, or the iron magnets of the machine, but the actions and reactions set up in the medium that surrounds them. We can readily imagine a dynamo constructed without any iron at all, and, although it would be very inefficient, it would illustrate the principle better than if the iron were there. Our methods of designing dynamos and motors have been very carefully worked out at the present time, so that if we spin the armature of a dynamo we can recover as electrical energy from it fully ninety per cent. of that supplied by hand, or water wheel, or steam engine; and with a motor we can get back ninety per cent. again in electrical energy, if it suits our convenience. We have, then, very excellent means for transforming mechanical into electrical energy and the reverse. The question that is the real subject of my lecture this evening is: What advantages can we gain by employing this double transformation of mechanical energy to electrical and then back again? In other words, when will we be the gainers by this process, in which there must necessarily be some losses. What compensating advantages can we find?

As sources of mechanical power we principally depend on steam engines, or on water-power; various other sources of power, prime movers we may call them, are now and then employed, such as gas engines, that have proved singularly convenient for some small work; petroleum engines, of much

the same general character; hot air engines, used principally for pumping; water motors; wind mills; and a few others. None of these, however, can compete on a large scale with the steam engine, or with natural water-power. Remembering this fact, that from steam or from water we must get our original supply of power, whether we use it directly or change it into the form of electrical power, let us look around and see under what conditions we can generate electrical current, use it in turn to produce mechanical power again, and be the gainers by so doing. One fact is really self-evident at the start; unless a given amount of power can be generated more cheaply at the point where the dynamo is driven than where the power must be finally utilized there is no direct economy in using an electric motor. In fact, the original power must be something like twenty per cent. cheaper at the dynamo than at the motor before the latter can compete directly with steam. I shall only consider steam or water driven dynamos as sources of electricity since all other methods of obtaining it are enormously more expensive; there is, in fact, no other practical way of getting electricity in quantity at all. We hope that there may be some day, but there is none now nor do we know in what direction to look for it; we hear rumors now and then of electricity obtained direct from heat, or directly from chemical energy, but it has been shown over and over again that such methods, although occasionally convenient, are immensely more expensive than the usual one.

Now, under what circumstances can we use the electric motor and gain by it? To answer the question on the moment, there are three general cases where the motor is distinctly economical. (1) When power is ultimately to be used in small amounts and we can take advantage of the cheapness of production on a large scale. (2) Where large amounts are needed but from accidents of situation power can be obtained much more cheaply at some distant point than at the place where it is desired for use. (3) When for any reason it is laborious and inconvenient to produce power at the point of consumption. The first case is an

exceedingly common one. Of a thousand places where power is wanted not less than 900 of them require it in very small amounts, less than a horse-power. A little power is needed to run a lathe here; in the next block a horse-power or two to operate a printing press, on a contiguous corner as much more to drive a bread mixer in a bakery; on the floor above it a half horse-power to drive a dozen sewing machines, and so on. Now we cannot produce power cheaply on a very small scale any more than we can most other things. In fact, the steam engine is singularly ill-fitted for use where the power required is but little. It is an unfortunate fact that steam engines are not usually well made in the smaller sizes. It is a very difficult matter to get an engine of less than about ten horse-power that will be as complete in all its parts, and will compare at all in mechanical perfection with larger sizes. With the electric motor the case is very, very different; an eighth horse-power motor may be as completely worked out in detail as one of a hundred times its power, and may be only slightly less efficient. Beside this, small engines are notoriously and unavoidably inefficient. All the sources of loss have a greater relative value as the size of the engine diminishes.

To pass from the general to the concrete, we can obtain a horse-power for an hour, that is, power enough to raise nearly ten tons 100 feet high in that time, by burning under our boiler about one and one-half pounds of coal, provided we are using an engine of the most approved type and of something like 500 or 1,000 horse-power. With smaller machines, of five horse-power or less, seven to ten pounds of coal would be required to do precisely the same work. So we can see at once that the outlay for fuel alone would be five to six times as great with many small engines as with a single big one, and besides, while the latter can be run by a very few men, the time of forty or fifty would be taken up in caring for the 100 small engines accomplishing the same work. Of course, not all the power generated in an engine that drives a dynamo reappears in the electric current; there are small losses all along the line of operation. The friction of the engine alone frequently

amounts to ten per cent. of its full power, and if it is running at half load we can see at once that twenty per cent. of its power would be consumed in friction, because as the load varies, the friction remains almost constant, being determined by the size and character of the working parts. Then, too, there are small, but unavoidable losses that come from heating the wires, from stirring up currents in the metal work outside of the wires from which we obtain it, in friction of the bearings in which the armature turns and in doing magnetic work on the revolving core of the armature, which is made up of thin discs of sheet-iron impaled upon the shaft. Taken together, all these losses amount to about ten per cent. of the full output of the dynamo, and all of them except one—heating—remain nearly constant whether we are taking much or little current from the machine. Consequently, if the dynamo is worked at only half its full power, these losses become of importance. Altogether, for each horse-power generated in the engine, one is doing very well to get back four-fifths of a horse-power in electrical energy from the dynamo. Still, taking into account all this, if we work on a large scale we can produce electrical energy on the whole very cheaply. It is not a difficult matter to figure out quite exactly, knowing the cost of coal, wages paid to employés and cost of plant, the net cost of each horse-power hour produced. A convenient figure to remember is that with a total capacity of 1,000 horse-power, not less than nine-tenths of it being regularly utilized and working twelve hours per day, the cost of power in the form of electrical energy on the line is almost exactly one cent for each horse-power hour; this includes everything—fuel, attendants, interest on the plant, depreciation of engines and dynamos, oil, waste, and water. In a 100 horse-power plant, working under just the same conditions, the cost would be a trifle over three cents. So you can see at once the advantage of working on a large scale in producing power, and it is evident that even if there were considerable losses in distributing it by means of electric motors, there would still be a very considerable advantage in so doing. If the 1,000 horse-power station of which we

were speaking is only used at a small portion of its full capacity, as I have just shown, some of the losses will be relatively much larger and the cost of power will be considerably higher. If, for example, only a quarter of the 1,000 horse-power were, on the average, to be used, the cost per horse-power per hour would be very nearly double that which I just mentioned. Now, it is very easy to see that electric power under these circumstances is much cheaper than using small steam engines at the places where the power is wanted, even allowing a large profit to the company that supplies the electricity—as a matter of every-day practice a common charge for running electric motors is $6 per horse-power per month; that is, less than twenty-five cents a day. In taking out, then, a small steam engine and replacing it by an electric motor, there is gain at every point; at the price I have just mentioned five horse-power could be bought for the daily wages of the man who would be required to look out for the engine and keep up the fire under the boiler.

The motor requires only occasional care, takes up very little room, is perfectly under control, and produces no dirt, dust, smoke or steam; and we must remember that with the engine there goes a boiler and a fire, both taking up room and requiring care, and somewhat objectionable on the score of safety. Where power is needed on a very small scale, a quarter or half a horse-power, the advantages of the motor become more and more manifest. No one would think of building or operating in a practical way a half horse-power engine, and if so small an amount of power were required, one would be driven to using a gas engine, or a water motor, neither of which can compete with the electric motor at the rate just given, unless in specially favorable situations. I hope, then, that I have succeeded in making it quite clear that in the case I have first mentioned, where power is required in small quantities, the electric motor is a very successful rival indeed of steam, because it is possible to produce power very cheaply on a large scale and distribute it by means of motors without excessive loss. Further, the electric motor is cleaner, more

convenient and simpler than any description of gas or heat engine which has yet been devised.

Now, let us take up the second case, where a large amount of power is required, for which a steam engine could be very well used, but where it would be rather expensive to operate one; these conditions exist in very many places where it is desired to drive machinery at some point at which it is inconvenient or expensive to get coal. A few miles away, perhaps, may be found an available water-power or a convenient coal mine or coal depot, where the price is low; in very many cases there will then be a decided gain in generating the power where it can be done more cheaply, and transferring the power as electrical energy through wires over across the country to the point where it is needed. Even supposing fifty per cent. to be lost in this transmission—a very large allowance—it often would be very economical. I know, for example, of a mining region in Colorado where power is necessary at each mine to drive stamp mills and other machinery, and where coal would be enormously expensive; five or six miles away from the centre of the mining district is a water-power, a mountain stream shooting downward 300 or 400 feet; this fall is now being used to drive dynamos that in turn will distribute power to every mine that needs it, and some of them are a dozen miles away. In any given case where power is wanted it is no very difficult matter to figure out how much it will cost to run an engine to produce it; and then a counter-calculation on the cost of obtaining the power from a distance electrically will show whether there will be gain or loss by using motors. In some cases, of course, there will be loss, in others, and many, a very considerable gain. So we are quite justified in saying that the electric motor may be cheaper than steam, even when the latter may be used on a large scale; the only condition being that we shall be able to take advantage of cheaper production by the ability electricity gives us to transfer power from a distant point. And finally we come to the third case, where electricity can be a successful rival of direct steam-power—in situations where it is inconvenient or troublesome to get power by any other

means. Fancy, for instance, trying to operate coal-mining machines and drills in deep mines by steam engines placed at or near the point of operation. The difficulties of ventilation and danger of fire simply prohibit successful use of engines, but wires can readily be strung and motors driven at 100 points from a distant power plant, and so we find that electric coal cutters and mining drills are gradually coming into extensive use, superseding the distribution of power by compressed air to a very large extent. Again, there are many situations where to use a heat engine of any description would be exceedingly inconvenient, or would bring down the wrath of the fire underwriters; here a motor can be safely boxed up and run in almost any position and under almost any circumstances with perfect security. Another example of the advantage that comes from convenience may be found in the many ways of using motors about the house to drive fans, to run sewing machines, to operate small pumps, and the like. Always and everywhere it is a power easily controlled, reliable and cheap; it can be led around corners by means of flexible wires, and moved about from place to place with the greatest freedom. It can be brought into play at a touch, and as easily stopped when the necessity for it is past; it can be regulated with the utmost nicety just to fulfil the demands that are to be made upon it, and can be trusted to go about its business almost automatically.

In all this we have seen that we must look upon electricity not as in any sense able to displace steam as a prime mover, but as an enormously powerful and convenient means of transferring power from one point to another with the greatest simplicity and very small losses. Probably for a long time to come by far the greater portion of dynamos will continue to be driven by steam engines, but so far as use at a given point is concerned the electric motor is a most successful rival of the steam engine. If the day ever comes, as it may, although we can hardly dare to look for it, when we can obtain electrical energy directly from coal, as we now obtain heat energy, the days of the steam engine will be numbered. As I have already pointed out elec-

tricity and heat are very intimately related. Every time we heat a piece of iron, for example, we stir up not only what we call heat, but also electro-magnetic energy, which is the form in which heat is radiated, and if we go on heating long enough we may raise the iron to a white heat, and recognize the energy in its familiar form of light. But all this is merely a dream of the future; perhaps it will be fulfilled some day, but until then we must depend upon our present dynamos.

In all that I have so far said you may notice with wonder that I have not alluded to the use of electricity as a motive-power; to the electric street railway, or to the magnificent possibilities of displacing the locomotive from its present supremacy. I have done this for the purpose of showing first, what can be done with the motor as a stationary machine, and now, of course, you will understand that very much that is true of the motor at rest will also be true of it when it is employed in moving itself and drawing loads. It is merely a question of delivering a certain amount of power at a given point, and it is in this field, really a modification of the second case of which I spoke, that electric power presents some peculiar and almost overwhelming advantages. The ingenuity of inventors has been sorely tried in attempting to build a steam dummy that will perform successfully on street-car lines. It is safe to say that nearly every such effort has come to grief, because an engine under such circumstances is not only working at a disadvantage, but is making itself excessively obnoxious to every one that chances to be on the same street. Here was the grand opportunity of the electric motor, that goes quietly about its business and can be applied at any one point, as well as many, provided it is duly supplied with current. So we connect the motor to the car axle and take current from a line strung overhead, by means of a travelling contact. For street-car service the steam engine is almost out of the question, except as it is used in the cable railroad, so that the motor really does not have it for a rival at all. Young as is the electric railroad the experience of the last three years has been sufficient to show that the horse

must go. Whatever might be the relative cost of an electric road and the same system operated by steam dummies. about the comparison with horses there is no doubt, and the strongest evidence of this is the fact that nearly 300 electric roads are in operation or building to-day, operating over more than 2,000 miles of track. Wherever the change has been made from horses to electricity there has been a positive saving that will grow greater instead of less as improvements are made. The question is often asked whether the electric railroad can rival the cable road; in other words, whether the power can be distributed more cheaply by electric motors or by continuously moving cables. The cable is at the best where the line is short, and generally heavily loaded, and under such circumstances is, at least, as economical as our best present electric railways. The improvements of a few years are likely, however, to reverse these conditions and where the line is long and not heavily loaded, the advantage is altogether on the side of electricity.

PROCEEDINGS
OF THE
CHEMICAL SECTION
OF THE
FRANKLIN INSTITUTE.

[*Stated meeting, held at the institute, Tuesday, February 17, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 17, 1891.

Dr. WM. H. WAHL, president, in the chair.

Members present: Dr. L. B. Hall, Prof. E. F. Smith, Dr. H. W. Jayne, Mr. H. Pemberton, Jr., Mr. W. H. Bower, Mr. A. W. Allen, Mr. Reuben Haines, Mr. R. A. Fisher, Mr. C. S. Boyer, Mr. H. Warren Shepard, Mr. Everett W. Frazer and four visitors.

Mr. Pemberton nominated Dr. F. A. Geuth as an honorary member of the section.

Dr. Wahl nominated Dr. Hans v. Strombeck, 104 Bank Street, New York City, and Prof. Joseph W. Richards, Lehigh University, Bethlehem, Pa., as associate members. The nominations were referred to the committee on admissions and the candidates were elected.

The treasurer read a letter from the actuary of the institute in reference to the payment of bills for subscriptions; the bills accompanying the letter were referred to the finance committee, which endorsed them for payment.

The treasurer also reported two names to be dropped from the list of members.

The resignations of Prof. R. L. Chase and Mr. Henry Warden were received and accepted.

Dr. Wahl, on behalf of Dr. Greene and himself, made a brief reply to a criticism upon their note on "Alloys of Sodium and Lead," in the *Chemical News*, of January 2, 1891. It was alleged in this criticism that the calculated densities for the alloys, given by the authors, were erroneous, and other figures were stated as the correct densities. The speaker showed wherein the critic had made an elementary mistake, and that the figures given by the authors were right.

Samples of the hydrofluoric acid bottles, patented recently by Prof. Edward Hart, of Easton, Pa., were exhibited.

Prof. Smith presented three papers, entitled, respectively, "The electrolytic method applied to rhodium," "The electrolytic determination of gold" and "The electrolytic determination of mercury."

They were listened to with marked interest and were referred for publication.

Mr. Reuben Haines presented a preliminary paper on the adoption of standards in interpreting the results of water analyses. He considered at length the variability of these standards among different chemists and in different parts of the country. The following figures representing the averages of thirty-four different determinations of uncontaminated waters were recommended as standards for pure waters in the neighborhood of Philadelphia:

	Parts in 100,000.
Free ammonia,	·0031
Alb. ammonia,	·0044
Chlorine,	1·19
Nitrogen in nitric acid,	·5075
Total solids,	12·57

After some discussion of this paper the section adjourned.

WM. C. DAY, *Secretary.*

IS SULPHURIC HYDRATE VOLATILE AT THE ORDINARY TEMPERATURE OF THE AIR?

BY GEORGE A. KOENIG.

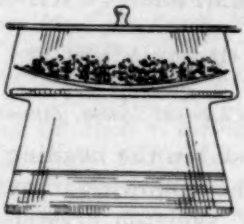
[*Read before the Chemical Section, January 20, 1891.*]

The question proposed in the heading of this communication arose under the following circumstances: I had received a number of skeleton crystals of metallic iron, which had formed in a steel block. To prevent them from rusting, they had been coated with shellac. Previous to the analysis, the shellac was removed by alcohol, and after drying on water-bath, the material not needed for the purpose in view was placed upon a perfectly clean watch-glass into a desiccator of the common pattern. The watch-glass was supported by an iron-wire triangle; its rims did not touch the sides of the desiccator. Upon the latter's bottom was pure sulphuric acid, about one inch deep. The lid fitted with a film of vaseline. The desiccator was then placed upon a shelf and remained there undisturbed for about nine months. Being needed for another purpose, I took it down, and found to my astonishment that both the iron-wire triangle and the iron crystals were covered with a clean white crust. What could this crust be? My first supposition reverted to the nitrous compounds frequently found

even in redistilled sulphuric acid. The crust was very coherent and peeled off in scales, leaving a bright metallic surface. I noticed that those parts of triangle, which had rested in immediate contact with the glass, were not incrusted. Likewise the watch-glass was bright and free from dust. The crust dissolved readily in distilled water, and proved to be composed of *anhydrous ferrous sulphate*. The test for nitric and nitrous acid resulted negatively. A quantitative analysis was not made under the circumstances.

How was this ferrous sulphate formed? Two possibilities only seem to present themselves:

(*a*) The acid has crept up the side of the vessel, and also from the triangle along the watch-glass, by capillary force. This hypothesis is not well sustained. In the first place



the sides proved themselves dry and clean, and so did the watch-glass. In the second place, one should expect the strongest action, where the triangle rested on the glass. But the fact obtained that these contacts were not corroded.

(*b*) The acid is volatile at ordinary temperature, similarly to other liquids with high boiling point, such as mercury. It might even be proposed that this volatilization is induced by the strong basic character of metallic iron; and the minute quantities volatilized, being immediately absorbed by the iron, there would not be any appreciable tension. There must have been produced, of course, a corresponding quantity of hydrogen, which would cause tension. I neglected to collect at once the gas in the desiccator and a test was thus impossible. On the other hand, the lid was not so tight, but that so light a gas could not readily escape.

There seems to me at present no escape from admitting the volatility of sulphuric hydrate at temperatures not exceeding 30° C., and more particularly in the presence of bodies exerting chemical attraction.

It will be probably of use to many to bear this fact in mind, when they employ sulphuric acid as a desiccating medium. I propose to institute—and, in fact, have already instituted, several sets of conditions, for proving or disproving this volatilization. They will necessarily be time-taking, as time seems the chief factor in the case.

CORRESPONDENCE.

On the Occurrence of Amberite, Ambrite, or Fossil Gum in a Coal-Seam at Hawakawa Colliery, Bay of Islands, New Zealand.

[*Presented at the stated meeting of the Section, held January 20, 1891.*]

U. S. S. Mohican, Opua, Bay of Islands,
New Zealand, September 13, 1890.

To the Chemical Section, Franklin Institute:

Gentlemen:—I have the honor of presenting to the Section, a specimen of coal from the Hawakawa Colliery, Bay of Islands, New Zealand.

This specimen I obtained at the mine, and for this detailed report of its characteristics, I am indebted to Mr. T. P. Moody, M.E., etc., member of the Geological Society of Australia.

Subject—"On the occurrence of amberite, ambrite, or fossil gum in a coal-seam at Hawakawa Colliery, Bay of Islands, New Zealand."

"I desire to place on record in the annals of our society a brief description of a most peculiar substance, which is often met with during the progress of coal-getting at this colliery. I refer to 'amberite' or 'fossil gum.'

"This is a gum or resin (mineral), and like most bitumens, it contains upwards of ten per cent. of oxygen, and, also, nearly the same proportion of hydrogen. It bears a close resemblance to Kauri gum.

"This peculiar substance, namely, amberite, imbedded in a coal-seam, has never been seen anywhere, but at the Bay of Islands, at Waikato and at Uhangarei, in the colony of New Zealand.

"In association and intermixed with these coals, especially at the Bay of Islands, large quantities are obtained. It frequently occurs in large masses, but generally is found in layers of small extent, and in 'nodules' extensively imbedded and disseminated over the coal face.

"It is thus described: Its hardness may be represented by two; specific gravity, 1·034; lustre, greasy; color, yellowish-gray, sometimes light and dark green and occasionally encrusted with a 'film' of carbonate of lime and spar; subtransparent; fracture, conchoidal; its chemical composition:

Carbon,	76·88
Hydrogen,	10·54
Oxygen,	12·77

"It is wholly insoluble in alcohol, ether, oil of turpentine, benzole and chloroform and dilute acid. It burns with a yellowish smoking flame. The ash contains iron, lime and soda.

"This amberite is an exudation from the Kauri pine tree, changed by chemical action, the only difference between it and Kauri gum being in color mainly. This amberite occurs in one seam of coal which belongs to the cretaceo-tertiary formation. In this series here there is only one seam of coal.

"The secondary formation of other countries, is, in this place, entirely absent, therefore the cretaceo-tertiary formation necessarily reposes upon the primary, viz: 'diorite slate.'

"In order that comparisons may be made as to other products somewhat bearing a resemblance to amberite, I give a few particulars of *re* gums or resins which are found in Europe.

"For instance, the amber of the Baltic Coast occurs in irregular mains, without cleavage. Its hardness is 2·25; specific gravity, 1·065 to 1·081; lustre, resinous; color, yellow; it is, however, sometimes reddish, brownish and whitish, often clouded; streak white; transparent; tasteless; electric on friction. Composition:

Carbon,	78·824
Hydrogen,	10·228
Oxygen,	10·900

"Then, again, the substance well-known to scientists as 'geomyricite.' This in color is 'wax-like' white; melts at 80° to 90° F.; is easily soluble in hot alcohol and ether, but slightly only in alcohol of 80 per cent. This is thought to be the resin exuded from some palm tree. Its composition:

Carbon,	80·59
Hydrogen,	13·42
Oxygen,	5·99

"This substance is almost identical with 'China wax.'

"In concluding I would briefly remark that all the gums referred to are, according to common opinion, the exudations of trees that flourished at the time the deposits were forming in which these substances are found."

Before closing I will state my experience with this coal as fuel for steaming purposes.

I have used this coal in the furnaces of the boilers of the United States steamer *Mohican* and find it to be a good coal for that purpose. Combustion is more rapid than with any other bituminous coal I have ever used.

It does not make any ash, but deposits a cinder or clinker upon the surface of the grate bars. This deposit is easily removed, as upon opening the furnace door to manipulate the furnace, the cold air being admitted, the cinder immediately detaches itself from the bars and is removed in the ordinary way. This cinder (a specimen of which I send) amounts to about eight per cent. of the coal consumed in the furnace. The smoke is of a light pearly-gray color.

Very respectfully,

J. M. EMANUEL,
Passed Assistant Engineer, U. S. N.

BOOK NOTICES.

Force and Energy, a Theory of Dynamics.—By Grant Allen. London: Longmans, Green & Co., and New York: 15 East Sixteenth Street. 1888.

In the preface to this very interesting work, the author states that "the perusal of certain dynamical treatises of Clerk Maxwell's, Tait's, Balfour Stewart's and Helmholtz's, suggested to his mind sundry profound difficulties in the current conception of the nature of energy;"* and he proceeds, with the utmost modesty, to suggest an entirely new theory, or new general view, of the mechanical processes of the universe.

Those who feel an interest in questions of what may be called ultimate dynamics, and who are not over-ready to cry "heresy" upon the appearance of every proposed innovation, will follow with pleasure the steps by which the author goes on to show how his theory harmonizes with those processes. The work is of special interest in connection with Prof. Lockyer's recently published views respecting the evolution of the universe through the aggregation of meteoritic dust.

Unfortunately, the new theory requires a new nomenclature, assigning new or modified meanings to terms already used, as will appear if we briefly summarize the author's propositions, beginning by quoting the opening paragraphs of the first three chapters, on "power," "force" and "energy," respectively.

"A POWER is that which initiates or terminates, accelerates or retards motion in one or more particles of ponderable matter or of the ethereal medium."

"A FORCE is a power which initiates or accelerates aggregative motion, while it resists or retards separative motion, in two or more particles of ponderable matter (and possibly also of the ethereal medium)."

"An ENERGY is a power, which resists or retards aggregative motion, while it initiates or accelerates separative motion, in two or more particles of ponderable matter or of the ethereal medium."

The "Forces" (or "Aggregative Powers") of nature, are the molar force of gravitation, the molecular force of cohesion, the atomic force of chemical affinity and the electrical force of unlike electric units.

* It seems most unfortunate that the author has not even given us a hint as to what these difficulties were.

"Every Energy (or "Separative Power") in the universe was primordially a mere statical separation of masses, molecules, atoms or electrical units. If there were no such power as force, every one of these bodies would have remained forever apart and immovable. But as forces draw together these mutually attractive material objects, their energy assumes for a moment the kinetic mode. The falling water, the moving glacier, the oxygen rushing to unite with the coal, each pass,* for a shorter or longer period, through the kinetic stage. As they aggregate, their energy is given off in some other form of motion, involving separation. But as they move about, they part with this motion in separating other masses or molecules, and the attractive force soon brings them together again."†

Energy, like force, is classified in the four "species:" molar, molecular, atomic and electrical, accordingly as it is exerted between masses, molecules, atoms or electrical units. It is also recognized under two modes, potential and kinetic. Finally, since motion may be either separative (as when a cannon ball is fired upward), aggregative (as when the ball falls again), or neutral (as in a spinning top or in a planet circling about its sun), so kinetic energy may be either of the separative, aggregative (notwithstanding the definition in chapter iii) or neutral kind.

But every such transaction is accompanied by a transfer of energy from matter to the ether. This energy, mostly in the form of heat, is radiated in all directions into space. Some small portion is necessarily intercepted by such of the heavenly bodies as it meets here and there; but the net result is a continuous transfer of energy from matter to the ether; in other words, a continuous waste of energy, so far as concerns man's uses. For even the small portion of radiated energy, which is thus intercepted by bodies in space, is only temporarily retained by them, and is sooner or later again passed on by them to the interstellar spaces.

This transfer of energy from matter to the ether is always accompanied, as a final result, by a further aggregation of matter. "The life-history of the material universe, as known and predictable by us, is the history of its passage from a diffused nebulous state to an aggregated solid state, through a vast number of intermediate stages, each one of which is an advance in aggregation upon the stage which preceded it."‡

The author traces this process of transference of energy and aggregation of matter, through sidereal, solar, terrestrial and organic phenomena, drawing many and apt illustrations from each of these spheres.

The orbital energy (our "centrifugal force") of the planets resists the (centripetal) force of gravitation, which would otherwise draw them to the sun. But this orbital energy is constantly being expended in, and reduced by, ethereal friction.‖

"Every particle of matter has inherent in it certain forces of which it can

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* Rather the energy of each passes.

† Page 76.

‡ Page 89.

‖ The author is careful to state that while *forces* (*i. e.*, attractions) may or may not be exerted between the particles of the ether, those particles are certainly endowed with *energies*.

never be deprived. The total amount of force, or aggregative power, in the universe is thus always a fixed quantity. This principle may be known as the persistence of force."*

"The total amount of energy, potential and kinetic, existing in the Universe is always a fixed quantity."† But while force is "rigidly bound up with the individual particles in which it is from time to time manifested," energy "can be transferred from one particle, or set of particles, to another." Hence while forces are spoken of as "persisting," energies are said to be "conserved." These two generalizations are summed up under the wider one of "the indestructibility of power," thus: "the total amount of power, aggregative or separative, in the universe is a constant quantity, and no power can ever disappear or be destroyed."

One force or energy may be prevented from producing its aggregative or separative motion, by the interference of another force or energy; as when gravitation is prevented from drawing a weight to the earth by the cohesion of a string or shelf supporting the weight, or as when the energy of steam in a boiler is restrained by the cohesive force of the particles composing the boiler shell. Such cases our author treats in chapters xi and xii, under the heads "the mutual interference of forces," and "the suppression of energies." But such interference may be terminated, and motion permitted, by the action of a "liberating energy," as when the energy of heat burns away the string or shelf which supports a weight and thus permits the latter to aggregate with the earth. Such liberation may also evidently be effected by a liberating "force," as well as by liberating energy, although this is not stated by the author. Thus, the shelf supporting a weight might be *pulled* away by the attractive force of a magnet, or by that between another weight and the earth, acting through a cord passing over a fixed pulley, as well as *pushed* away or burned away by energies.

Vegetal organisms, during life, act chiefly as *accumulators* of energy; for they absorb water and carbonic acid; decompose them into their constituent elements, or into "energetic compounds" (such as hydrocarbons), and retain those elements, etc., in a state of separation or potential energy, using only small portions of this energy (transforming it into the kinetic mode) in germination, efflorescence, etc. By decay after death, or by absorption into some animal organism, the stored-up potential energy is kinetized, liberated and turned out upon the ether, and the previously separated elements are allowed to recombine.

Animal organisms, on the contrary, are *expenders* rather than accumulators, of energy. They devour the "energetic compounds" (hydrocarbons, etc.) evolved by vegetal organisms, and thus cause them to evolve kinetic energy (forming part of the life of the animal, and afterwards going to waste), and to aggregate by recombining with oxygen to form stable or non-energetic compounds.

It thus appears that what we commonly call the "*de*-composition" of organic matter, is more properly described as *re*-composition; for it consists in a

* Page 39.

† Page 41.

reuniting (under the force of chemical affinity) of atoms of carbon and hydrogen, with oxygen from which they have previously been separated by solar energy acting through the organism.

The foregoing is, we believe, a fairly complete outline of the new theory.

A radical defect of the present treatise. and one which prevents it from doing justice to the theory, is the entire absence of the quantitative feature. We are left quite uninformed as to the *dimensions* of the author's "forces" and "energies." Our old-fashioned "force" may be measured in pounds, and our "energy" in foot-pounds; but we are not told in what units these new "powers" are measurable, or even whether they are or are not both measurable in one and the same unit. On page 11 we are told that the amount of the energy required to lift a given weight to a given height is the measure of the resistance offered by gravitation. From this it would appear (if, as we generally understand, "resistance" is the equivalent of "force") that the "force" of gravitation is to be measured in *foot*-pounds; but, if so, how are we to measure the force of gravitation acting between the earth and a stone resting upon it; and how are we to understand the statement, already referred to, that "every particle of matter has inherent in it certain forces, of which it can never be deprived?" In several places, too, the author speaks of kinetic energy as being equivalent to motion. Is motion, then, to be measured in units similar to the foot-pound?

We are inclined, notwithstanding the author's explicitness in general, to attribute the occurrence of these somewhat puzzling expressions to an occasional, and by no means unpardonable, lapse in the matter of rhetorical accuracy; for instance, in several places we find *mist* referred to as a form of *energy*, whereas probably no one knows better than our author that it is a form of *water*.

There are many cases of every-day occurrence in which it seems difficult to decide how to apply the author's distinction between "force," as aggregative power, and "energy," as separative power. For instance, when a man lifts a dumb-bell from the ground to himself, he no doubt does so by means of "energy," for which he is indebted to the chemical aggregation (within his body and under the action of the force of chemical affinity) of carbon and oxygen which had previously been separated by solar energy, acting through the medium of vegetal processes. And in lifting the dumb-bell he is plainly counteracting the "force" of gravitation between the earth and the dumb-bell, and separating them in spite of that force. Yet, on the other hand, the man's energy manifests itself primarily as a *contraction* of his muscles, or as an *aggregation* of their particles, for which, by the author's definitions, we require, not energy but force. Besides, while he is separating the weight from the earth, he is drawing it toward himself, or causing aggregative motion. Have we here then a case of (animal) energy overcoming the force of gravitation, or are we to regard it as a case where one force (gravitation) is overcome by another (muscular attraction)? And how must the answer to this be modified for the case where the man, after *pulling* the dumb-bell from the ground to the level of his shoulder, extends his arm and *pushes* the bell both from himself and from the earth? Even here, we must remember, the extension of the arm

and the further separation of the dumb-bell and the earth are produced by the *contraction* of muscles, viz: those at the back of the arm.

Except for the necessary difference in nomenclature, it does not clearly appear wherein the theory here advanced conflicts with those usually held. Each is but a method of describing and explaining occurrences upon whose actuality all are agreed; and while two or more such methods may be correct, that one is the best and the most convenient, which most completely harmonizes with those occurrences and leaves the fewest phenomena unaccounted for. In the absence of quantitative data it would seem quite impossible to institute any satisfactory comparison in the present case.

It is worthy of note that the new theory has been made by Mr. Edward Clodd the foundation of his "Story of Creation,"* a popular and compendious, but serious, exposition of the doctrine of evolution. Mr. Clodd emphasizes the convenient distinction between energies as *pushing* powers and forces as *pulling* powers, and further notes the more radical distinction between energies as powers that may be stored (since they exist whether they act or not), and forces as powers incapable of storage. T.

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Franklin Institute.

[*Proceedings of the stated meeting, held Wednesday, February 18, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 18, 1891.

JOS. M. WILSON, president, in the chair.

Present, 210 members and thirty-one visitors.

Additions to membership since last report, eighteen.

The Secretary announced the death of Edward Lang, LL.D., F.R.S.E., of Edinburgh, Scotland, an honorary member of the institute; also the resignation of Prof. Wm. D. Marks, as a member of the Committee on Science and the Arts.